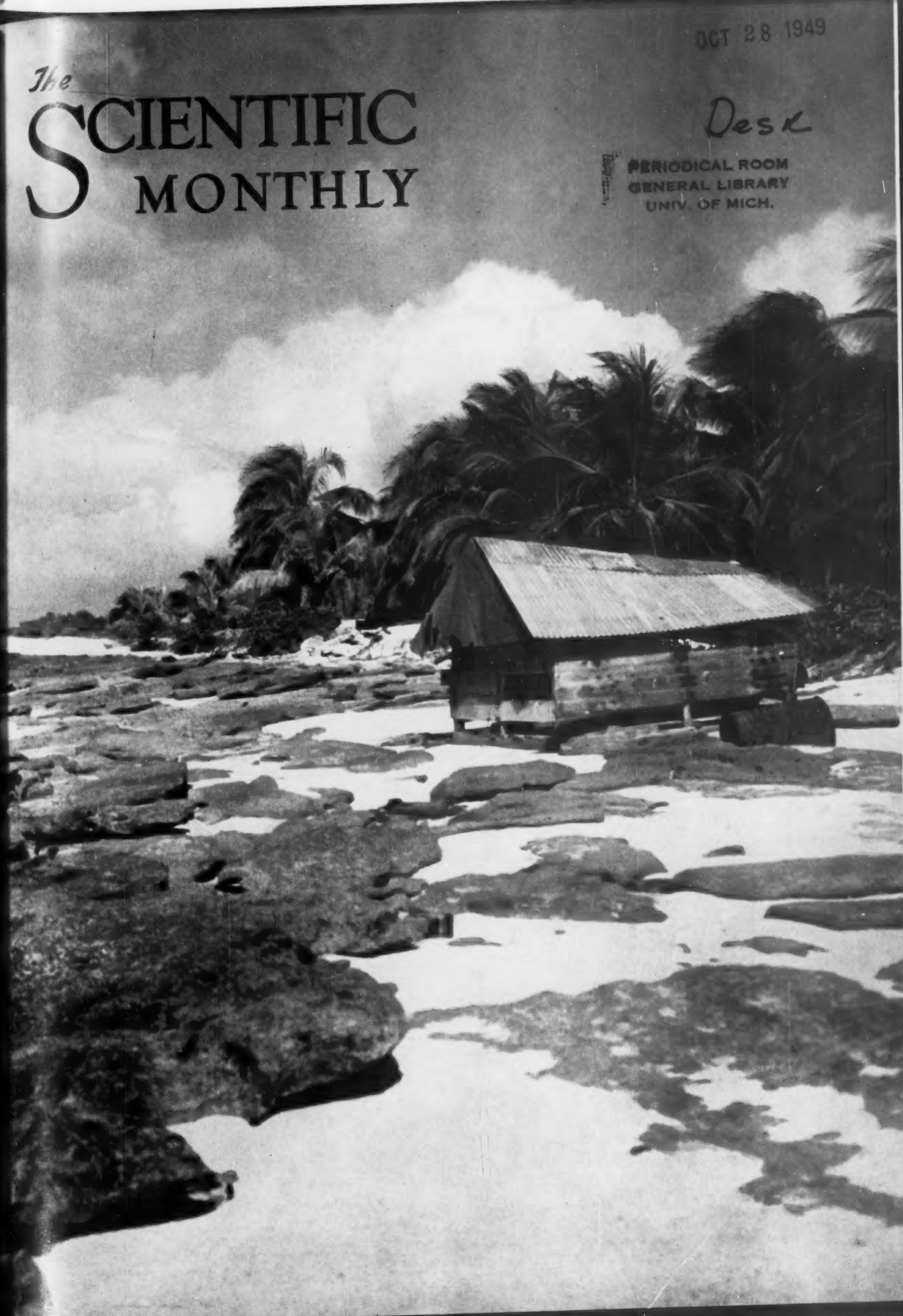
The SCIENTIFIC MONTHLY



THE

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SCRUB TYPHUS, OR TSUTSUGAMUSHI DISEASE

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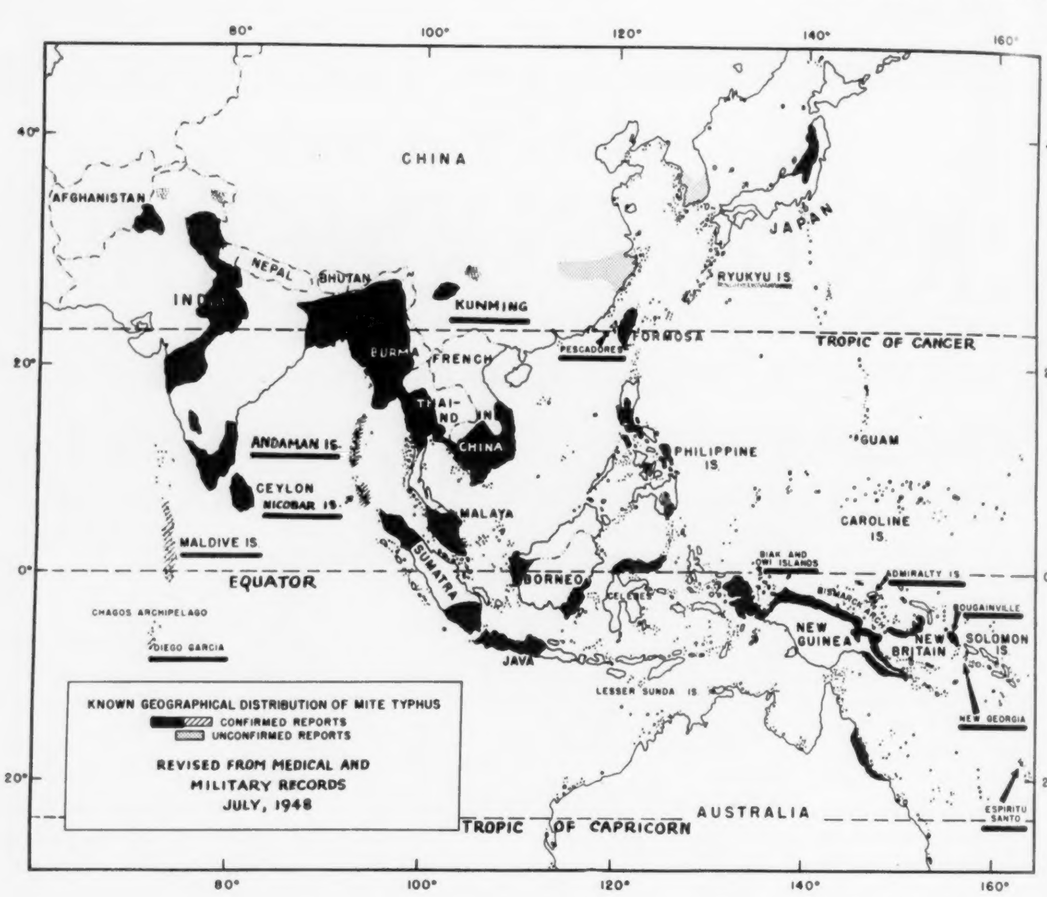
A SURPRISING number of important diseases of man and his domestic animals, which have eventually proved to be conveyed by insects and their allies, had long been reputed by the inhabitants of affected localities to be actually so caused. Yet, more often than not, these beliefs and "superstitions" were confirmed slowly and reluctantly by medical and scientific observers.

Such has been the story of flood-river fever, or tsutsugamushi disease, in Japan. Smith and Kilbourne's epic report (1893) demonstrating the first arthropod transmission of pathogenic organisms (the agents of tick-borne cattle fever) appeared in the same year that Kitasato gave the first serious credence to the early beliefs of Japanese farmers that "flood fever" was actually caused by the bites of another acarid, called by various colloquial names, including *akamushi* ("red bugs," or "mites"); this became the eventual species name for the vector in Japan. Certain of these minute creatures were also called *tsutsugamushi* (from early Chinese, meaning "poisonous or dangerous creature"). Thus the classical name of the malady was derived from the vector itself even before its transmitting role was proved.

Kawamura cites early Chinese medical literature as suggesting occurrence in ancient times of a similar malady along rivers in China, but Japanese accounts supply the first accurate descriptions of the disease. Recognizable medical accounts appeared early in the nineteenth century in Niigata and Yamagata prefectures. The first Western accounts are given in a letter of a medical missionary, Dr. Theobald A. Palm, published in the Edinburgh *Medical Journal* in 1878, and in a German description the following year by Baelz and Kawakami in Virchow's *Archiv*. Both mention the beliefs of local inhabitants of causation by *shimamushi* ("island bugs"), associated with river fever.

Tanaka, a physician in northern Japan, was the first serious student of the mite vectors. In 1899, he published descriptions in German, and inaccurate figures of the accused mites, which he soon followed with reports that there was more than one kind on the local voles, including the same kind of "thin-haired red bugs" that were found on man. By the time of his final report in 1930, some six different kinds of these mites had been described on Japanese voles, or meadow mice, which required (and still do) investigation as to their relationship and relative importance in the epidemiology of the disease in the endemic areas along the rivers of northwest Honshu. It is generally conceded, however, that the original *akamushi* is the important vector to man in Japan.



Nowhere else in the countries of the Pacific-Asiatic region where the disease was subsequently discovered—Formosa, the Philippines, India, Burma, Indo-China, Thailand, Malaya, Indonesia, New Guinea and adjacent islands, and Australia—had the epidemiology involved the peculiar restriction to immediate river margins that it had previous to this year* in the classic areas in Honshu.

As the picture continues to unfold in these other regions, it becomes increasingly evident that the epidemiology of this disease is pre-eminently a reflection of the local ecology of the major mite hosts—*Microtus* and possibly *Apodemus* in Japan and various species of *Rattus* in the other areas, with birds probably playing a part in the introduction of infected mites into new localities.

Although similar infections were known under various colloquial names outside Japan, it was not until the past decade that their identity with the classical tsutsugamushi disease was confirmed following pioneer studies on "scrub typhus" in Malaya and "pseudotyphus" in Sumatra. The study of strains and cross-immunity in laboratory animals, and the Weil-Felix test, provided the chief laboratory evidence that settled the confusion caused by observed clinical variations in different areas.

As early as 1908, Ashburn and Craig had reported two cases on Samar, Philippine Islands, which showed similarities to both tsutsugamushi disease of Japan and Rocky Mountain spotted fever of Montana, but it is remarkable that incontrovertible evidence of the presence of scrub typhus in the Philippines was not forthcoming until American reoccupation in 1944–45 revealed foci on six of the major islands.

The causative agent. Scrub typhus is caused by a typical rickettsial agent (minute, bacterialike organisms), which invades the cytoplasm but not the nucleus of infected host cells. It is thus more closely related to the agents of endemic and epidemic typhus than to those of the spotted fever

* Only this past year has an endemic area been proved outside the classic ones, this on the slopes of Mount Fuji (*Historic Report, 1948*, 406th General Medical Laboratory, Tokyo).

group. It does not pass ordinary filters as does the agent of Q fever and has not been grown on artificial media. Rich growths have been obtained in the yolk sacs of embryonated chicken eggs and in the lungs of infected laboratory rats and mice, but vaccines prepared from these sources have been disappointing in meager field trials.

Beginning with a supposed gregarine, a number of "organisms" were accused as etiologic agents of scrub typhus by early Japanese investigators. The first unequivocal reference to its rickettsial relationship was discussed and clearly figured in 1924 by Nagayo and his colleagues, but they and others later admitted that Hayashi in 1920 probably saw rickettsiae in some of his preparations, from which he described *Theileria tsutsugamushi,* with a purported malarialike cycle in the blood. Hayashi's species is therefore transferred to the genus *Rickettsia* in the revised Bergey system, though it is difficult to accept the editors' belief in Ogata as the original reviser, since the latter still claims (correspondence 1949) independent discovery of the organism which he named *Rickettsia tsutsugamushi* in 1931. This and several other specific names are synonyms of *Rickettsia tsutsugamushi* (Hayashi), including the latest proposal of Hayasaka of "var. *tropica*" in a postwar printed military report of the disease in Japanese forces in Burma, Siam, Malaya, and Indonesia.

Photomicrograph of *Rickettsia tsutsugamushi* in infected cell of Giemsa-stained smear of infected animal tissue. (×1500.)

This rickettsia is more difficult to stain than most species, unless first fixed with a defatting agent—methyl or absolute alcohol. It is readily demonstrated in the cytoplasm (but never in the nucleus) of cells in Giemsa-stained smears from the body cavities of infected mice or from yolk sacs of infected chicken eggs.

Fortunately for diagnostic purposes, a fortuitous discovery in Malaya in 1926 revealed that serum of persons convalescent from scrub typhus agglutinated an accidental mutant of *Proteus* OX_{19} which was labeled *Proteus* OXK (after Kingsbury, who provided the strain that presumably mutated). This was a variation of the original so-called Weil-Felix reaction for certain other rickettsial infections. Although nonspecific as a serologic test, the OXK reaction is now considered to be diagnostic in endemic areas when high titer is obtained, or a rising titer occurs in serially drawn serum samples from a patient. In certain areas, such as the Kunming district of southern China, epidemiological considerations differentiate louse-borne relapsing fever, which may also elicit a rise in OXK agglutinins.

The mite vectors. Chigger mites of the family Trombiculidae occur all over the world and are peculiar among mites (which are really minute ticks) in that only the larval stage is parasitic, and then always on some vertebrate. The unfed larvae are so minute, on emergence from the egg, as to be scarcely visible to the untrained eye; they have been observed to penetrate the mesh of a coarsely woven sock. After completing engorgement on a host, the larvae drop off and continue development as free-living nymphs and adults in the soil.

Only recently have techniques been developed for mass rearing of any species. Crucial disease-transmission studies in the laboratory have thus been hampered but are now being undertaken. Preliminary tests with Dr. Dale Jenkins, of the Army Chemical Center, have not succeeded in demonstrating adaptation of the pathogen to transmission by North American chiggers, although actual ingestion of the organisms was accomplished.

Only two kinds of trombiculid mites have so far been incriminated in transmission of scrub typhus to man. Named *Trombicula akamushi* and *T. deliensis,* they are so closely related systematically, with morphological intergradation, that some tax-

The "dorsal topography" of *Trombicula deliensis*, one of the two known vectors of scrub typhus. The other, *T. akamushi*, differs microscopically only in having more spines posteriorly. (This photomicrograph was awarded honorable mention in the First Annual International Photography-in-Science Salon. Greatly magnified.)

onomists have considered them varieties of the same species complex. It is to be hoped that the new breeding techniques will provide evidence to clear up this moot question.

The first was shown by the Japanese to be naturally infected, and to occur on voles coincidental with human cases in the warm months in the endemic areas. Since the parasitic larvae customarily attach only to one host in a given generation, it was considered axiomatic that the infection passed through the subsequent non-parasitic nymphal and adult stages and via the eggs to the next generation, but the data substantiating this are very meager for either of the above mites. Furthermore, reports are conflicting regarding demonstration of infection in wild-caught adults and nymphs. Although infection was readily recovered from larvae off rats in recent Malayan studies, no infection could be demonstrated in considerable numbers of nymphs and adults from the soil of foci where the rats were trapped.

T. deliensis was named for Deli in Sumatra by a Dutch investigator who first discovered it there. During the recent hostilities, great impetus was given to determining the extent of chigger mite occurrence, and surveys have revealed that both this and *akamushi* are widely distributed in all the major endemic countries, though one or the other may predominate in a given locality—sometimes to the exclusion of the other, as on certain of the smaller islands. A few specimens indistinguishable from *T. deliensis* have only recently turned up in collections from Japan, where it had been thought that only *T. akamushi* was the vector to man.

At least seven other species of trombiculid mites have been found on Japanese voles; two of them occasionally attack man and have also been reported to have caused infection when injected into laboratory animals. Because of their minute size even when engorged, it is, however, hardly possible to be certain that no *akamushi* were present in the injected samples. A relative of these mites, new to science, has just been discovered on voles in the new Mount Fuji focus in Japan.

Circumstantial observations cast suspicion on certain other species during the early studies in Sumatra, and in both the New Guinea area and northern Queensland during the war, where species (other than the above) that will bite man were the only ones found incidental to local human infection. These included certain species of mites that are capable of causing "scrub itch," an irritation of the skin appearing promptly after the bites, which is entirely unrelated to scrub typhus. This was encountered by troops in several Pacific areas but appeared strangely absent from Burma and the Philippines.

The larvae of both vectors contain pinkish pigment even before feeding, which is the basis for the widespread illusion that attached mites are ingesting blood. Like other trombiculids, however, they feed only on lymph and tissue fluids, through a peculiar tube which penetrates the subdermal tissues, from the bladelike, embedded mandibles. Continuing its extension during feeding, this tube may become longer than the body of the mite itself, but its origin has never been satisfactorily explained.

About midway in the incubation period, or during the first mild symptoms, the site of an infected mite bite often ulcerates to form a so-called eschar. The percentage of patients showing this sign varies in different areas, for unknown reasons, but not in relation necessarily to the virulence of local strains of infection. Differenti-

ation of scrub typhus from tsutsugamushi disease on this basis is no longer accepted.

The vertebrate "reservoirs." Following the demonstration of natural infection in tissues of mite-infested voles in Japan, attention was focused on native murid species in other endemic areas. The indigenous rat fauna of the Pacific-Asiatic region is very rich in species. Recovery of strains from various local species has been reported in Formosa, Burma, India, Malaya, Java, New Guinea, and Queensland. Five species or varieties of rats were represented. Other species are undoubtedly susceptible also, and many have habits that make them good mite hosts.

The domestic, or commensal, forms of *Rattus* are apparently involved only occasionally. They are seldom found infested with vector mites except under special environmental conditions, as in the suburbs of Calcutta or the farmyards of the Pescadores Islands west of Formosa. Under special conditions, other local faunal factors may also intervene, as in parts of Burma and India, where

Raised, blisterlike reaction about a chigger attached for thirty-six hours at the base of a hair on a human forearm.

tree shrews have been found infected in nature. The susceptibility of the marsupial bandicoots is still unconfirmed, but these animals have been accused and could be an important factor in the antipodes.

In view of Hayashi's report of the susceptibility and natural infection in certain native Japanese birds, it is surprising that known foci had remained confined to river margins in northwest Honshu until the Mount Fuji focus was encountered. Bearing on this consideration are data of the writer showing persistence of the agent in the blood stream for varying periods in sparrows and domestic fowl, the longest (twenty-six days) in turkeys. A captive Malayan jungle cock, which the writer had under observation in Kuala Lumpur in May 1948, had demonstrable infection in its brain nineteen days after injection with a laboratory strain.

Since ground-frequenting birds, in particular, become heavily infested with vector mites in endemic areas, the implication of birds as carriers is obvious for spread from a given focus, or to isolated South Sea islands such as Bat, Ponam, and Espiritu Santo, on which foci were encountered. During surveys in a 398-acre cutover rubber field in Malaya where infection was not known until after the war, quail were found to be much more heavily mite-infested than were indigenous rats, and infection was recovered from mites off two of these quail. These birds could, therefore, cause extension of the focus in this and neighboring fields more rapidly than local rats, which are much more restricted in their migrations.

For laboratory tests, white mice are the animals of choice, though North African gerbils and field mice of the genus *Microtus* are also readily and fatally infectible. Under the impetus furnished by combat problems with this disease, many strains of the rickettsia have been isolated from human, rat, and mite sources in most of the known endemic countries. It is apparent that there is a wide zoological adaptability of the agent. Nevertheless, it is generally believed at present that the rat-mite-rat cycle, fortified by transovarial continuity between mite generations, constitutes the fundamental mechanism for natural maintenance of infection in most tropical foci.

Unquestionably, the mites act as important reservoirs, but the demonstrated fact of rickettsiae persisting in tissues of experimentally infected rats for many months requires further investigation of murids in the reservoir role also. Dutch investi-

gators have recently reported recovering infection from experimental guinea pigs nearly two years after inoculation.

The epidemiological picture as brought out during hostilities. As has been implied, the ecology of the disease in the tropics was found to differ from the flood-river fever in Japan in that it involved environments harboring a variety of native rats rather than field mice. One of the tragedies of military operations is that they have always provided mass exposure of susceptibles to various endemic diseases in combat zones. Knowledge of both the geographic and the ecologic distribution of scrub typhus was greatly augmented as a result of war activities and the occurrence of cases in troops in out-of-the-way places.

A type of coarse grass (*Imperator cylindrica*) known as *lalang* in Malaya was particularly suspected as dangerous before the war. Troops again encountered the disease in fields of this grass in New Guinea (where it was known as *kunai*), in the Philippines (*kogan* grass), and in parts of Burma and India. This grass is also found along the endemic river margins in Japan, where it is called *susuki* and *yoshi*. Local epidemics resulted in the troops and in postwar labor in other grass associations, particularly a forage grass (*Pasapalum conjugatum*) in Burma, Siam, and Malaya; the seed of this grass are probably attractive to the rats as food. The grasses in some of these fields were virgin stands; in others they comprised secondary invaders in abandoned rice, cane, and tapioca fields.

Coconut groves, neglected during the Japanese occupation, developed an undergrowth providing good rat harborage along the coasts of some of the Philippine Islands, New Guinea, and adjacent small islands, which also produced several major military outbreaks. These doubtless also will be the source of civilian cases in the postwar reclamation period. In Malaya, important foci of this type shifted from prewar oil-palm estates to cutover rubber and other previous war-emergency food-growing areas.

Although it was suspected in several areas that cases were contracted by soldiers in primary rain forest, there was usually a history of previous contact with more open terrain during their incubation period. A large proportion of cases occurs near sea level or in low inland country, but foci are known in mountainous terrain in New Guinea, Formosa, and India, where infection has been found as high as six to seven thousand feet in Kashmir and the Kumaon Hills. There may be interesting ecological significance in the preponderance of the foci north of the equator. These foci extend well north of the Tropic of Cancer but do not even reach the Tropic of Capricorn to the south. The factors affecting the distribution of the vectors and their hosts are obviously concerned.

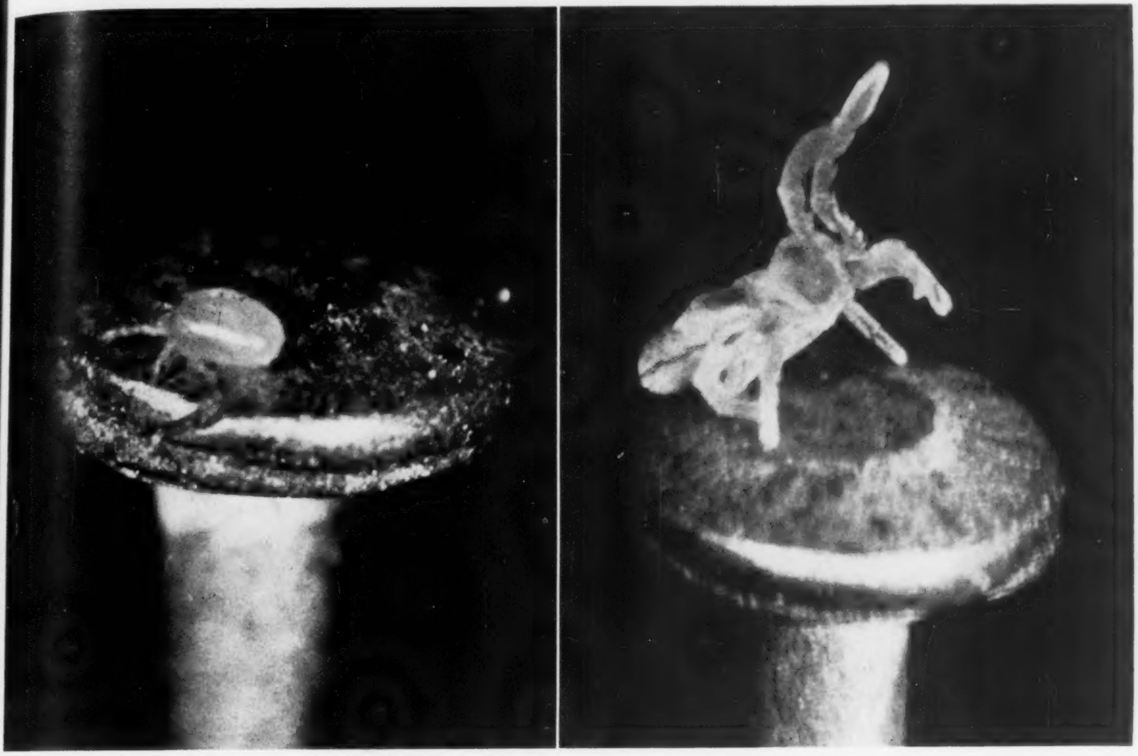
It thus has become increasingly evident that wherever the local environment encourages the rat-vector-rat natural association, there is potentiality of human infection. Such conditions have been shown to exist even in primary jungle in New Guinea and Burma, so that it is probable that "silent" foci exist in many areas as yet unvisited by the white man.

The most marked seasonal variation in incidence is in Japan and northern Formosa. In some parts of western Burma and India, the annual dry season adversely affecting the local mite populations is reflected in a drop in case occurrence also. Experience of the Australian and American forces, on the other hand, indicated that fluctuating exposure and troop movements rather than seasonal factors accounted for variations in incidence in Queensland, Indonesia, the Philippines, and northern Burma.

The disease. Scrub typhus is a severe, prostrating, typhuslike infection, often with an eschar at the site of the mite bite. It is typically characterized by swelling of the lymph glands, a skin rash, headache, loss of appetite, muscular aches and pains, and temporary damage to certain tissues and organs, particularly the circulatory system, including the heart. Permanent damage to the heart has been claimed as a result of symptoms observed during late convalescence, but this has since been discounted.

The incubation period was sometimes determined rather exactly when timed by invasion schedules. The average is around ten to twelve days, but instances as short as four to six days have been observed.

Fatality rates vary in different areas. In Japan, as high as 60 percent has been reported, whereas only 0.06 percent mortality occurred in the Owi-Biak epidemic of 1,469 American military casualties. The usual fatality rate in the tropics has been between 10 and 20 percent. Duration is prolonged, with a conservatively estimated average loss of sixty to seventy man-days per case in military personnel. Some convalescents are rehospitalized with weakness and heart and nervous symptoms; in such cases, convalescence is slow. Two or even three relapses during convalescence were recently



Chigger mites are minute. *Left*: A fully-fed specimen on the head of a pin. *Right*: The adult mite is bright red. It does not attack animals, but lives free in the soil. (Photoflash caused recoil.)

noted in Malaya. On the other hand, during epidemics, mild febrile episodes have been observed which were diagnosed as scrub typhus only by serological means two or three weeks after return to full duty, in some instances without requiring hospitalization.

The fact that second and even third attacks after varying periods have been seen suggests that immunity is not as solid as in some other typhuslike diseases. Recent discussion has even considered evidence that immunity is dependent on "premunition," or the persistence in the tissues of the host of asymptomatic infection such as envisaged for certain viruses.

The antibiotic chloromycetin, recently shown by Smadel, Woodward, and their associates of a U. S. Army team to be dramatically effective in treatment of this disease in field trials in Malaya, has proved to be the most effective of the drugs tried to date, and tests have indicated it is also of value in temporary suppression, or chemoprophylaxis against infection during and following exposure.

The drug can be administered by mouth, has been found not to be toxic for clinical use, and to result in defervescence in twenty-four to thirty-six hours after an administration period as short as twenty-four hours, as well as to shorten markedly the period of hospitalization. Laboratory tests with experimental mice also suggest that another new antibiotic, aureomycin, may be equally effective in treatment.

Control as practiced before and during the war. In the absence of a proved vaccine or, until very recently, of an effective drug against the agent itself, control measures, past and present, may be divided into two classes, both directed against the mite vector: temporary efforts to protect exposed persons from mite bite, and more permanent manipulation of the environment to reduce mite prevalence.

Until the advent of effective acaricidal impregnants, miteproof clothing was ineffective in Japan and, in addition, impractical for the tropics. Impregnation of uniforms with dimethyl phthalate by American forces, and with dibutyl phthalate by the Australian forces, both in the Southwest Pacific, was proved feasible, but not widely adopted in this or the Burma theater, owing chiefly to the lateness of use in the campaigns.

The Australian hand-treated uniforms were re-

ported to have reduced incidence and to have resisted several launderings, remaining lethal to invading mites. Other improved acaricides were soon developed. Much the most effective of these resistant chemicals is a now well-known insecticide, benzene hexachloride (BHC), but it has proved to have dangerous toxic properties to the wearer. The newest of the clothing impregnants on the U. S. Army Quartermaster supply shelf consists of 45 parts of benzyl benzoate (a well-known scabicide), 45 parts of dibutyl phthalate, and 10 parts of an emulsifier. The effectiveness of this acaricide as a clothing impregnant has been demonstrated incidental to field studies in proved hyperendemic foci in Malaya, where discontinuance of wearing of treated clothing during exposure resulted in infection among volunteers.

Barefooted Tamil laborers "ring-weeding" young rubber stumpage in a focus of scrub typhus. Postwar Malayan rubber estate near Kuala Lumpur.

The development of agriculture in the tropics, however, can hardly depend in practice on this means of protection for relatively primitive labor, owing to minimum of dress and the habit of going barefoot in the fields. To insist on the wearing of footgear and treated socks by Tamil labor on rubber plantations in Malaya, for example, would probably result in detrimental psychological reactions outweighing the advantages of protection afforded for purposes of production. A further difficulty recently demonstrated in practice in Malaya also involves the human equation—native wearers of treated clothing may become lulled into a sense of false security and become lax in their care to maintain proper miticidal barriers even under supposed close supervision. Each of two native field assistants, presumably indoctrinated, became infected after over two months of field work under equivalent conditions with two Europeans, in constant company with them in the same foci, who did not become infected. Furthermore, one of the latter became infected in the same areas when treated clothing was later intentionally omitted.

The other alternative, of reduction of indigenous mite population in a given local environment, is practical where heavy machinery is available, such as on a military beachhead or about the immediate environs of human habitation on plantations in endemic areas. The vector species have been found to be very sensitive to exposure of soil terrain to drying and to sunlight. Mites on trapped local rat hosts have been found to practically disappear where ground litter and other protective cover has been cleared. But again this is economically im-

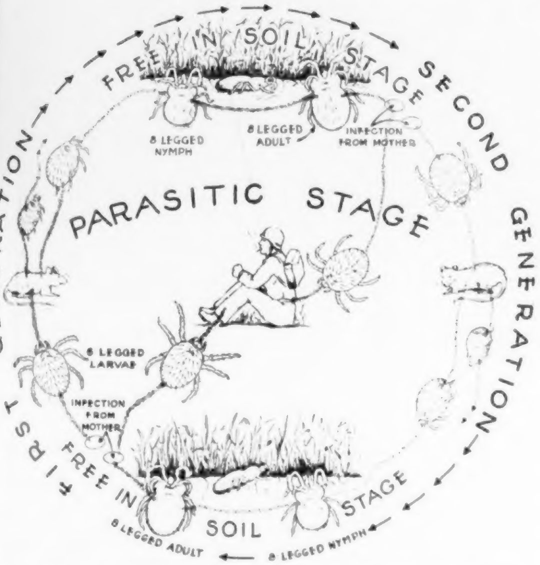
Anglo-American and Asiatic volunteers exposed in a fo[cus] in Malaya during field trials of chloromycetin as a p[ro]phylaxis.

practical where extensive acreage must be cleared by hand labor. Burning of mature grasses is feasible too late in the season in Japan, usually dangerous, and only partially effective. Under tropical conditions, rapid regrowth occurs, affording repopulation by immigrant rats. Rat control could aid, but it is hardly practical on the extensive scale that would be required for plantation areas.

It also has been shown by the Orlando investigators of the Bureau of Entomology and Plant Quarantine that among other acaricides, crude benzene hexachloride is a superior and durable soil disinfectant against chiggers when applied at the rate of 50 and 25 pounds of toxicant per acre. Sulphur and DDT were much less effective. This again would be an economically feasible control measure only under very restricted circumstances.

Future lines of research. Some fundamental questions relating to the epidemiology and control of this disease remain to be answered. Are there strain differences in the etiologic agent of the order of the differences between epidemic and murine typhus as suggested by recent serological evidence? The answer to this question could bear on the feasibility of a practical vaccine. How satisfactorily can military and agricultural needs be met by the new drug chloromycetin when it is brought into adequate production, from points of view of both treatment and suppression? How effective, comparatively, in the field is another antibiotic, aureomycin? Even granting their effectiveness, will these drugs be economically desirable for use in large areas of the Pacific tropics where production is dependent on cheap, usually barefoot, native labor?

Much remains to be learned regarding the vector question. What, if any, species of mites other than *akamushi* and *deliensis* are capable of harborage and transmission of the rickettsial agents, and, if they exist, what is their importance epidemiologically? Present reports of other vector species need confirmation. Is there a qualitative or quantitative change in the passage of the disease agents



The "rickettsial stream" of *R. tsutsugamushi* in nature through two generations of mites. (Courtesy U. S. Army Medical Museum.)

through the nonparasitic nymphal and adult stages? What is the percentage of progeny that acquire infection from an infected female via the egg? Is this a sufficient explanation of maintenance of foci, or must there be a proportion of new lines of infection started from infected vertebrate hosts? Since rats are known to carry persisting rickettsiae for a year or more, the answer to the above questions and to whether rats can act as effectual asymptomatic carriers to infect successively attaching mites will have a bearing on the emphasis to be placed on local control measures. The adaptation of newer rearing techniques to laboratory studies of the vector mites appears imminent and will open up many profitable lines of research in spite of the hazards involved. The reduction of these hazards inherent in the experimental handling of these minute creatures is an important step.

Problems of the vertebrate "reservoirs" also require much additional observation, particularly the role played by birds. The ecology of the vertebrate hosts probably has a vital relationship to the distribution of the vectors, and hence to explanation of the peculiar, and still enigmatic, spotty focal restriction of the disease in a given endemic area. Some differences in ecological factors are probably involved in explaining occurrence of foci in the mountains of Formosa, the Philippines, and India, and on isolated Pacific islands, while the disease has apparently failed to spread beyond the immediate river margins in northwest Honshu in Japan except for the focus near Mount Fuji. There are also important lacunae in our knowledge of the susceptibility of important mite hosts in various localities and of the epidemiological significance of such unusual hosts as tree shrews found naturally infected in parts of Burma and India. Almost nothing is known regarding the environmental and food requirements of the free-living nymphs and adults of the vector species.

These are some of the problems that serve to emphasize the need for much more work in elucidating the epidemiology of scrub typhus. Answers to at least some of them may be forthcoming from small teams of British and American investigators now in the field in Japan and Malaya.

Photomicrographs by N. J. Kramis.

ETHICAL SCIENCE

HENRY MARGENAU

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BEING untrained in ethical theory, I think it very likely that all I have to say has already been said in more adequate and more pleasing ways. If this calls for apologies, I gladly offer them. What matters today is not whether ideas on ethics are original, but whether they work in our world. Democracy, as we understand it, has tended chiefly to *dissolve* old standards, and has shown itself thus far none too competent to create new basic norms. Moreover, it has set itself against accepting all authoritarian standards, because they violate our concept of freedom. And it is becoming fairly evident that the idea of freedom alone, no matter how prolific of special "freedoms" it may become, is never going to settle in a positive way how we shall decide the most basic and the most intimate issues of our lives. Under these circumstances, "ethical science" has a strong appeal.

The purpose of this note is to illuminate the impressive parallelism that exists between the traditional problems of ethics and those of science. In many cases, the analogue of the ethical problem has been solved in science, and the question is here asked whether the known solutions may not contain suggestions useful for ethics. But before proceeding to this central theme, I shall restate briefly the position that makes such an inquiry possible.

"Ethical science" differs from "scientific ethics" as that term is often employed: it is not a method of solving ethical problems by reference to the *content* of science. Science, we hold, will never replace ethics; its subject matter is not continuous with moral actions, for the *ought* is foreign to the factual *is* of science. No attempt will, therefore, be made to revive Stoicism with its precept of living according to nature, or any more modern version of naturalism which makes believe that if one squeezes science hard enough it will give norms for action. The time for this illusion is past. Science has its vogue; its successes are impressive enough in its own field. Yet by itself it is powerless to mold the behavior of men. So far as action is concerned, science never rises above *hypothetical* function: it will build houses and automobiles and planes, *if* man decides that such devices are worth having; it will feed millions, *if* it is decided that millions ought to be fed; it will destroy life, *if* men desire it to be destroyed. But the desire, the decision, rest on grounds that are inaccessible to scientific routine.

There is only an appearance of truth in every argument which denies this fact. Typical is the reasoning of hedonism, which goes as follows: "It is a general law of nature that all living things seek pleasure or happiness in some form. Therefore, to be in accord with scientific law, it behooves us to promote by our actions all conditions which enhance pleasure or happiness in ourselves or in others." And thus it seems that a normative maxim has been extracted from science. There are two important errors in this piece of reasoning. The first is inherent in the word "therefore" placed at the beginning of the second sentence. If we grant the premise that all living things seek happiness, then, being a living thing, I *must* seek happiness as a matter of fact, and there can be no question about the validity of this ethical doctrine. Yet we know that the conclusion does not follow in this simple syllogistic way, for it requires assent of a sort never forced by logic. I can, after all, promote unhappiness if I wish.

There is, in fact, an unstated premise whose inclusion would make the argument formally correct. It should run: "Most living things seek pleasure. I wish to conform to the character of most living things. Therefore I will seek pleasure." In this form it becomes apparent that the second premise, "I wish to conform," etc., is independent of the first, and can be independently denied. Only the first premise comes from science; over the second, science has no jurisdiction.

Leaving the second premise unstated is but one fault of the hedonistic argument. The other is the falsity of its major premise. To say that pleasure-

seeking is a scientific law is misunderstanding science. For if it is a scientific law no one can violate it, as has already been mentioned. The law of gravitation prevents no one from throwing stones upward and is not violated by this act, nor does one contravene it by walking off a cliff. Indeed if seeking pleasure were a law of nature, it could, because of that very circumstance, never function as a basis for ethics. Careful analysis shows that the statement in question, far from being a law, is but a definition of pleasure, or of happiness. For it amounts to making pleasure the motive of all actions: martyr and sensualist alike are driven by pleasure. This settles the matter—and at the same time it makes pleasure, or happiness, ubiquitous enough to be unimportant to ethical concerns.

The state of affairs described is well illuminated by a corresponding situation in physics. The relation between science and ethics is not unlike that between the first and the second law of thermodynamics. The first permits a very great variety of physical processes, many of which are never observed. No violation of the first law is involved in the prediction that a pail of water should boil when placed on a cake of ice, or that the cloud which rose over Hiroshima should reassemble itself and form the bomb from which it issued. It is the second law, not derivable from, but superimposed on, the first which makes thermodynamics into a reasonable discipline. Now replace the first law by the whole of science; you then need a "second law" to produce a reasonable state of human affairs. That second law is ethics.

But it is one thing to admit that science is impotent to sire ethics, and quite another to claim its irrelevance for ethics. For, after all, science has developed a methodology the success of which ethics may well envy. I propose that ethics "copy" those traits of scientific method that can be conducive to its own progress, not because these traits are scientific, but because they represent the best that human ingenuity has at present to offer. The inveterate humanist who regards such proposals as undignified and insulting has had his day; he will regain his stature only by reflecting seriously upon the risks involved in maintaining his aloofness from science.

To implement our suggestion, it becomes necessary to state briefly what the essential method of science appears to be and how its analogue would function in ethics, and to draw attention to the parallelism which exists between the fundamental problems of moral philosophy and those of the philosophy of science.

I

By science we shall mean *exact*, or deductive, science. This restricts all considerations to disciplines which have a clear mathematical or logical structure; it leaves aside such descriptive correlational sciences as the natural histories, sociology, and, to a large extent, economics. From the point of view here taken, this exclusion is not a neglect or an adverse judgment of their significance; it merely acknowledges that these sciences are probably not yet ripe for ultimate methodological analysis, that they have not yet reached the state of fertility and the power of prediction to which they quite obviously aspire.

It is widely believed that all sciences are correlational and inductive. A theory, it is often said, is merely a generalization of the facts of immediate observation: intelligent guesses from particular to more general situations, sanctioned by maximum probabilities, are the methods whereby science proceeds. I feel that this view cannot be maintained in the face of modern physics, but shall not argue the point in detail. The entities of the astronomic and of the atomic domain lay claim to a status in the scheme of reality which cannot be justified by declaring them to be inductive generalizations of immediate experience; they draw sustenance from metaphysical principles which give them strength beyond the trickle of vigor they derive from the concatenation of probabilities linking them with immediate facts. For the likelihood that an electron exists, or that the universe expands, is found to be practically zero when it is compounded by probability laws from the individual probabilities of the numerous specific propositions that compose these statements. Theory, it seems, is stabilized by two kinds of requirement: one, that the facts of observation be *deducible* from it without fail; and, two, that it obey certain postulated norms (such as continuity, causality, simplicity, etc.). By satisfying the second requirement—which is meaningless for the facts of immediate experience—as well as the first, theory comes to grips with reality in a more significant way than does mere observation.

If this appraisal of the function of theory in science is in error, the relevance of everything this note is meant to offer at once collapses. Ethics is then also a thicket of correlated behaviors which the anthropologist is expected to unravel. The reader who rejects the thesis that physics, for example, is both deductive and confirmative in its procedures, need not go on, for he will see in the subsequent remarks only the grotesque inflation of a basic error into a pretentious ethical theory.

But the sympathetic reader will recognize science as a great postulational enterprise forever adjusting itself to the demands of contingent facts. We postulate the laws of arithmetic, construct number fields, and draw from them conclusions which conform to certain parts of our direct experience. The postulates—in this case the laws of numbers—are found so successful in a limited domain as to convey the impression among the uncritical that they have a kind of absolute truth, quite independent of their logical genesis; hence it came as a shock to many minds when the properties of atomic entities refused to obey the laws of arithmetic and required matrices, not numbers, for their description. With the recognition that electrons are not billiard balls rapidly gaining ground, we are prone to smile and think what fools we've been to think that the axioms of arithmetic should be valid for all possible experience! And, continuing to muse in this vein, we become aware of the unfortunate circumstance that even now we have not been able to invent a calculus for handling ideas, which, by their apparently spontaneous generation, their refusal to be additive, their disappearance through forgetting, defy the laws of all normal calculi.

In other fields of science, axioms of other sorts lead to metaphysical and empirical satisfaction. There are Newton's laws of motion, Schroedinger's equation of quantum mechanics, any one of the so-called relativistic cosmological models, Pauli's exclusion principle, the laws of genetics. At present all these are to be regarded as *different* hypotheses regulating diverse parts of our experience. Most of us hope, it is true, for an all-embracive future theory which will unify these various postulates, but we do not wait for it before proceeding with the important business of science. And it is remarkable that so much could be achieved with the use of rather tentative postulates which have no a priori sanction! Hence it should be noted how such success became possible.

Axioms,† that is, norms of thought, fully formulated and clearly understood, were accepted for methodological purposes as true, were consistently adhered to with utmost care in all deductive procedures. Yet the possibility was always left open for modifications of the initial premises when facts required them, and without undue concern over eternal verities the scientist frequently recast his axioms for greater conformity with the contingencies of direct experience. Nor did he see an inconsistency in this procedure.

† By axiom, or postulate, we mean here any basic (unproved) hypothesis which has deductive fertility.

What he regards as inconsistent is failure to honor his tentative commitment to a formulated norm of thought during the process of deduction and empirical verification. He calls such inconsistencies errors of reasoning. Their avoidance is not always an easy matter; it often involves a degree of tedium and of determination rivaling the disciplined steadfastness of moral integrity. Also, there are often personal advantages to be gained through scientific error, which becomes a moral lapse when detected but not corrected. Indeed, there are places where the distinction between scientific and moral error becomes rather thin and where there is an overlapping of issues, as in situations governed by a scientific code of ethics. At any rate, scientific sin is not commensurate with *doubt as to the axioms* of a given discipline: it is the *failure to honor a commitment* to a set of maxims, whether they are ultimately tenable or not.

Having clearly formulated its basic principles, science goes forth to the second phase of its methodology and derives from these principles all conclusions which experiment is able to test. Naturally, of course, the formulation of principles is initially carried out with an eye upon their likely survival under test; the psychology of discovery draws heavily upon suggestions coming from unexplained facts. But this does not alter the logic of the procedure here under study. The second stage of scientific performance, deduction of specific theorems and prediction from postulates, is largely dominated by the analyst (the theoretical physicist, the applied mathematician), who prepares the scientific material for the guidance of the confirming and often discovering experimenter.

We have now arrived at the third phase in which the predictions issuing from the fundamental laws are confirmed or confuted. Confutation, even in a single significant instance, calls for rejection of the premises; confirmation, on the other hand, results precisely in the effect which this word describes: it renders the hypothesis firmer, though not indubitably true. The process of testing laws is intricate and beset with great complications; it is not always a matter of simply *seeing* whether a prediction is correct. For instance, to verify a law of quantum mechanics usually requires far more than an observation; it calls for numerous interpretations which are possible only by an appeal to the theory itself. But, here again, I do not wish to go into particulars; it is sufficient to emphasize that science was not born with obvious criteria by which it might demonstrate its truth. Indeed, the significance of tests had to be de-

veloped along with its own deductive formalism.

To summarize: science has three logical (not necessarily historical!) stages: (1) postulation of principles or laws; (2) prediction on the basis of laws; (3) confirmation of the predictions and thus validation of laws. The temporal sequence of scientific investigation may combine these stages in any order. It may start with a flagrant confutation of a prediction from accepted laws, and lead, by an inquiry that travels back and forth between all the stages, to the discovery of new principles. Cases in which discovery actually moves in the order of logical sequence, from 1 to 3, are citable, but rare.

For one who understands science in the manner here set forth, it is exceedingly difficult to see why ethics should not operate in a similar way. The elements of method which constitute a science are present in ethics as well: there is a moral code, or, shall we say, there are moral codes, in perfect analogy with scientific postulates; there is the task of explicating and expounding the code, of drawing from it all its consequences, and this is the exact counterpart of such scientific procedures as solving the laws of motion under special conditions. Finally, there arises in ethics very clearly the problem of confirming the code, of seeing whether it is the best attainable, or whether it is in harmony with the philosophy espoused by its practitioners. Let us follow further the suggestions contained in this parallelism. Let us postpone for once the ready conclusion that the scientist talks nonsense whenever he leaves the subject of his métier. For, although the specific conclusions I shall draw from these considerations may be entirely in error, it is nevertheless possible that a recognition of the tripartite structure of ethics, by revealing more clearly the problems that are to be solved, may prove beneficial to the progress of moral philosophy.

One need not be a partisan to the view that ethics has the structure of a science to see the harm which has been done by the persistent failure to respect the distinctions between the three phases. The first and the third are most frequently confused. Somehow, men do not wish to accept a moral code unless its validity can be demonstrated in an a priori manner to begin with. They act like the pseudo scientist who will never accept anything but the consequences of theories he already knows to be correct. They are like the physicist of the early century who rejected the quantum theory, because it violated what he called common sense. Now it is obvious that *empirical* validation cannot be had beforehand, whatever one's philosophy of the a priori may be. As a consequence, the person who confuses the first with the third methodological stage of ethics is driven to seek accreditation of moral doctrine by divine agencies, or at any rate to look for its documentation as an eternal verity deep in the nature of things. He does not realize that he is asking or looking for something absolutely unique, something which no other discipline requires as a starting point. The axioms of science, for example, can be accepted and are indeed accepted *prior* to their proof, for a variety of reasons. The genius who conceives them may regard them as inspirations; some have traced them to a divine origin. To others they are suggested by what they expect to happen; others again adopt axioms because of their logical simplicity or their mathematical elegance. Whatever the motive, one adopts them with complete respect for their integrity as guides in scientific conduct, even if one wishes ultimately to prove them wrong.

What I advocate is a postulational form of ethics, one that gets going before its code is definitely known to be "true."

Our schematism has the further advantage of calling attention to the need for verification of ethics, which is usually either confused with the other phases or forgotten. A moral system does serve a purpose, and clarity as to the general conditions under which the system could qualify as valid must at some time be attained. At present one finds concern over this problem predominantly in rather vague queries having to do with the effect of particular actions upon an individual's happiness, or upon society. Such issues, it seems, will have to be discussed in a much larger setting and probably on more abstract grounds. Perhaps Kant's categorical imperative, with its detached concept of duty, is the nearest approach to scientific ethics in its attitude toward confirmation at the present time. Furthermore, stage three calls upon us to envisage confirmation of ethical doctrine as a large historical venture, perhaps as a vast social experiment, ranking in scope and duration with astronomical enterprises whose completion extends over many years. Validity does not come the easy way, by soul searching or by miracles.

A word should be said about phase two, which I have badly termed "explication." In our time it seems to be very much the neglected phase (except among legal minds, where, however, it takes on certain aspects that remove it from the ethical realm). But we recall that *casuistry*, the established name for the activity under discussion, denotes what was once a flourishing and widely practiced subject. It has sunk into disrepute, prob-

ably as a result of the spread of relativism in ethical philosophy and of the attendant loss of force of all moral codes. But certainly the point of view here adopted confers upon casuistry an importance quite commensurate with the other phases. Explanation of the meaning of norms, in terms of individual behavior should, I believe, rate higher in public education than it does today.

II

Several powerful arguments stand against our view, which takes seriously the structural parallelism between ethics and science. Most damaging in its public effect is the thesis that the subject matters of science and of ethics are wholly different in kind, and that this difference is destructive to all comparisons between them. Science deals with objective facts, ethics with the subjective behavior of men.

If this is meant to assert that ethics is a far more difficult subject than any of the highly developed sciences, I can find no fault with it, for then the argument merely urges a more emphatic and forthright adoption of the methods which are known to be successful in the simpler fields. If, however, the argument implies that ethics will *never* be understood, then I think it is dangerous. Fortunately, it makes little sense when thus construed. The reason most commonly given for the belief in a fundamental disparity between natural science and the science of man is this: A natural object is unaltered by man's speculations about it. Whatever conclusions he draws from its presence do not effect it in the least. But knowledge is a primary and most important factor in man's behavior. There is what has been termed "a strong interaction" between subject and object in the science of ethics, and the objective description of natural science must fail in consequence.

But this argument loses its point, because it is no longer true that science must limit its scope to "objective" matters. Atomic physics and some parts of biology are domains where scientific procedures, measurements, and so forth have as severe an effect upon the observed system as a moral action has upon any behavioral situation. And yet these sciences have succeeded in developing exact deductive theories. Indeed, what seemed, in the classical stage of science, to set it apart from man's reasoning about his own volitions and actions is rapidly disappearing, and a new appraisal of the problem of objectivity is badly needed.

Then there is the bearded supposition that moral decisions matter greatly to the individual who makes and subjects himself to them, whereas scientific ones do not. The implication is this: If I decide not to steal I shall be out a certain sum of money (a most unpleasant prospect!), whereas in making an error in a calculation the effect does not concern me directly. I defy anyone who has had both experiences, and who says that the rigors of scientific discipline, especially to one as unskilled in science as most of us are in moral matters, are less arduous than the stamina it takes to resist temptation. When carefully inspected, such arguments invariably break down.

More important and perplexing is the problem of generating a code of ethics, which is of course the starting point of the entire enterprise. Shall we continue historical practice and wait until some great moralist, by the persuasive force of his personality, sweeps us in his train? Or shall we let the state impose its moral dicta in the manner in which it enforces its laws? Laws are, after all, a crustlike moral code near the periphery of ethical concerns. Or shall we stage a convention of elected moralists and appoint them to the task of formulating a code? The fact that such possibilities, when thus concretely phrased, sound silly is indeed an indication of our immaturity in the field of ethics. I am not endeavoring here to make a contribution to these difficult problems, except to say that they must and can be faced.

III

In the following, we shall avail ourselves of the facilities offered by our parallelism, to comment upon some of the traditional paradoxes of moral philosophy. It will be seen that each of them has as its counterpart a certain problem in the philosophy of science, which in most of the instances to be recorded has been solved. It seems plausible to assume that the solution offered on the side of science, if it has a correlate on the side of ethics, is at least suggestive of a way in which the ethical problem can be attacked. The procedure will be illustrated under five queries.

1. *Is ethics an autonomous discipline?* This question has already been answered in part. The subject matter of ethics cannot be wrung from science. Though both have similar structure, each has its own postulates or norms, and each requires specific acceptance or, if it seems more adequate to use a moral term, personal dedication to norms. This disposal of the problem does not preclude the possibility that ultimately the axioms of ethics may be reducible to those of science, just as chemistry and astronomy have become branches of physics. The point is that they are not thus reducible at

present. In my own opinion, the reduction is not likely to be achieved, and the worst attitude would be to wait for it.

2. Is ethics to be based upon a theory of values? Ethics and the theory of values are closely linked by tradition, and the weight of much authoritative writing is behind this tradition. To brush it aside without careful consideration seems unbecoming, nay, frivolous—yet the verdict of scientific ethics is very clear on this point. For the problem at issue has an exact corollary in the dispute over the relevance of *facts* in science. Translated, the question becomes: Is science to be based on facts?

Science has often been defined as a systematization of facts. But the bluntness, the inadequacy of this formulation, become apparent as soon as the meaning of fact is examined. If fact is considered synonymous with all that is found to be true on inspection, the definition is empty and valueless, because it includes too much that is trivial. A complete catalogue of mere matters of fact is not a science. And if the meaning of fact is restricted to *significant* fact, the definition begs the question. For it is science itself which confers significance; prior to the adoption of principles, no fact of observation, however trustworthy, has claim to importance. A person who painstakingly weighs every grain in a large pile of sand makes no contribution to science unless he is perchance verifying or refuting some hypothesis or law. In the accurate sense, then, a fact of science is a datum with relevance to postulates. And it can easily happen that a datum takes on relevance long after it has been discovered, although this is by no means assured. Hence a scientist would be misguided if he went about collecting facts indiscriminately in the hope that laws will be found someday to reward his labors.

As principles are prior to fact, so are ethical norms prior to values. Psychologically, anything can take on value. There is nothing intrinsic in human behavior, aside from anthropological statistics, that allows a qualitative distinction to be made between good actions and bad. The psychology of a dope addict's craving is quite the same as normal hunger. Furthermore, our belief in the efficiency of moral education is a clear demonstration of the conviction that things which appear good can be made to seem bad, that motives can be strengthened or weakened, that things of little value to the untutored can take on great value with understanding and with use. All this implies the logical priority of *principles*, of *codes* of behavior.

What the theory of values is patently meant to do is to persuade people that certain standards of behavior are more acceptable than others. Values are offered as prizes, neatly wrapped, for the acceptance of norms. But upon analysis, the circularity of this procedure emerges every time: Values are merely products of some code of behavior which the advocate of the code cherishes or wishes to propagate. To recognize the secondary nature of values and to concentrate upon the primary issues, namely, the norms which generate values, cannot but be of benefit to ethics.

But surely, the reader will say, a code of ethics is not something one offers for acceptance in its own right, as an abstract thing from which all blessings flow. Are we not treating this so-called code too much like a mathematical formula? Here I feel that perhaps we are not treating it enough like one. At any rate, we are losing sight of the ominous fact that, when it is so treated, enormous consequences for good or evil can follow. Dictators have seen this point. Good teachers know it, for they rely on personal example (which always sets a norm directly), rather than upon the secondary use of values. In fact, the moralist who is reduced to reasoning about values, has usually lost his grip on the pulsating flow of human behavior.

Although unreasoning acceptance of norms is possible—indeed, is far more widely practiced than we citizens of a democracy like to believe—it is not advocated here. Nor is it what the parallelism between science and ethics demands.

A postulate cannot be separated from its consequences, and it is because of these consequences that it is introduced. This must be true in ethics as well.

What we need, however, is greater emphasis on these postulates and less on values. Too few of us know what are the guiding principles of our behavior. Yet we all know our likes and dislikes, our values.

3. Are ethical standards relative or absolute? In answer to this question the scientist is expected to sing the praises of the theory of relativity, which, as many writers see it, has invaded the field of ethics. This belief rests on a misunderstanding of the physical lawfulness summed up in the theory of relativity, which asserts a kind of *in*variance for the workings of nature that is completely falsified by the customary phrase: all things are relative. The postulate of relativity has an air of absoluteness, of independence, shared by very few generalizations in any science. One cannot draw upon it in an endeavor to prove that standards are relative.

Science does in fact not give comfort to the relativist in ethics, for the laws of nature remain

unchanged as we pass from one situation to another. The so-called principle of uniformity of nature, assumed in one form or another throughout science, operates to deny the type of relativism usually envisaged in ethical applications. If we follow its lead, we arrive at the clear decision that norms do not permit variform interpretations under different circumstances.

But axioms do change in time. As the scientist learns more about his universe, he takes occasion to review his axioms and his laws, bringing them into better conformity with his experience. Older formulations are seen to be only approximate, or indeed cease to be valid. This self-regulating dynamism is one of the wholesome features of scientific progress, and if we read its implications aright, it teaches the lesson that ethics, too, might well temper its insistence on eternal truth and consider an occasional review of time-honored standards. If this is what we may understand by relativity in ethics, then the success of science affirms it.

4. *Are ethical standards derived from more elemental and primary considerations?* We have already indicated the answer to this question. As norms are the counterparts of axioms, they are themselves necessarily primary. Human acquiescence to them can be wholehearted and effective despite their logical parthenogenesis, and one can become convinced of their validity as thoroughly as one longs for an ideal. In a practical sense, we must of course acknowledge the psychological force of auxiliary motives, be they as base as hope for reward and fear of punishment, or as refined as the love of fellow-men. To enlist such motives is the task of the practical moralist, who, even as an ethical scientist, does not violate his integrity by their use. For does not the teacher of arithmetic, though he be fully aware of the postulational nature of the number system and of its limitations, appeal to instances (marbles, fingers, etc.) in which it is helpful? He finds it quite unnecessary to warn the pupil against employing the multiplication table with Heisenberg's matrices.

5. *Do we accept a standard of behavior because of the inherent qualities of the standard, or in anticipation of its practical consequences?* In other words, do we perform good acts for their own sake, or because of their effects on ourselves and others? Both alternatives have been fervently avowed by different schools of ethics. More common, perhaps, is adherence to the latter thesis, for every form of eudaemonism, every proposal to seek the highest good, is an affirmation of it.

Science is afflicted by a very similar controversy between *causal* and *teleological* description. On its outcome a final verdict cannot be rendered. Causality seems to be winning the race inasmuch as teleological principles, like Fermat's, can be translated into differential equations which imply causal relations. In conclusion, then, we may probably say that scientific ethics, as we have defined it here, favors the view that moral action is compulsory on man for its own sake, and this is in satisfactory accord with the postulational attitude demanded by our parallelism on other grounds.

The loosely coordinated remarks of this note were not intended to develop a systematic theory of ethics. They are particularly defective in their omission of almost all reference to the problem of confirmation, whose treatment is by far the most difficult on the side of science, and therefore presents challenging aspects to ethics as well. My purpose was to sketch an approach which appears to have some promise.

THE PROBLEM OF CORAL REEFS*

H. S. LADD and J. I. TRACEY, JR.

Dr. Ladd, who has studied coral reefs in the Pacific for the Bishop Museum and the University of Rochester, has been with the U. S. Geological Survey since 1940. Mr. Tracey, who is also on the staff of the Survey, was, with Dr. Ladd, one of the geologists who did research on the coral reefs of Bikini (Operation Crossroads).

DURING the past ten years much new interest has been aroused in the ancient and controversial problem of coral reefs. Some of this interest may be traced to the publication of papers describing prewar researches, part of it to work carried on during the war when reefs and related structures were intensively studied, and part to postwar investigations; also, in this decade additional deposits of petroleum were discovered in a number of ancient reefs, and this aroused the interest of petroleum geologists in the subject of reefs in general. A number of recent papers have been directly concerned with the origin of existing reefs. Some of these studies dealt primarily with reef surfaces and submarine mapping, others reported on deep drilling into existing reefs, and still others dealt with geophysical data on reef foundations not yet reached by the drill. Plans for additional work in all these fields are being made, and it seems appropriate to review the general problem very briefly, stressing recent developments and pointing out what appear to be promising lines of approach for future investigations.

THE REEF PROBLEM IN EARLY DAYS

No doubt the first travelers ever to see living coral reefs observed some of their peculiar features and speculated on their mode of formation. Actual records of this kind go back at least to 1821 when Chamisso[4] published his observations; this was sixteen years before Charles Darwin[9] presented the first statement of his famous theory. Pre-Darwinian speculations aroused little interest, though they may now be interpreted as preliminary rumbles of an activity that was to reach volcanic proportions some decades later. Darwin's observations led him to suggest that a fringing reef growing on a slowly subsiding foundation could be transformed into a barrier and finally into an atoll by organic growth. Seldom, perhaps, has a new idea been more quickly or more widely accepted than was Darwin's. Additional investigations by J. D. Dana[8] gave considerable support to Darwin, and it was not until 1863 that the first real opposition was brought forth by Carl Semper,[36] who had worked in the Palau (Pelew) Islands. Semper concluded that atolls could be formed in areas of elevation, their lagoons being formed by solution as the reefs grew outward. He took account of other factors also, but his ideas on the whole were the antithesis of those held by Darwin and Dana. Later, others followed Semper's lead; the period from 1880 to 1910 was a particularly prolific one, and many able investigators—John Murray, J. J. Rein, H. B. Guppy, W. J. L. Wharton, J. Stanley Gardiner, Alexander Agassiz, and others—took issue with Darwin and proposed a variety of alternate theories. The chief support of Darwin's theory in this period came from the deep boring at Funafuti[16] in the Ellice Islands, but the results of that boring were given very diverse interpretations.

In 1910 R. A. Daly[6] called attention to the speculations of A. Penck on the relations between Pleistocene glaciation and the forms of coral islands and outlined in some detail the glacial control theory. In this and many later papers Daly explained how coral reefs could have originated on the banks left close to sea level at a time when sea level was low because of the removal of waters to form glacial ice on the land. Later, as the glaciers melted, the reefs grew upward with a rising sea level. This theory, like Darwin's, won many supporters from the start, and it seems fair to state that it has had at least as many adherents as its older adversary. Indeed, since the glacial control theory became established, no student of coral reefs has felt justified in ignoring its postulates entirely.

Many of those who have studied coral reefs—including some that antedate Darwin—believed that a pre-existing platform of some sort was essential to the development of reefs. Of these, some postulated a change of sea level as necessary, others felt such a change was not essential.

* Published by permission of the Director, U. S. Geological Survey. Photographs by J. I. Tracey, Jr.

Theories calling for antecedent platforms have not been as widely held or as vigorously defended as either of the two mentioned earlier.

THE THIRTY YEARS' WAR

During the next thirty years—1910–39—the subsidence theory of Darwin and Dana was vigorously defended, particularly by William M. Davis, and Daly developed the opposing glacial control theory. In 1928, six years prior to his death, Davis[10] published a volume entitled *The Coral Reef Problem* which, though it may be characterized as "a full and unprejudiced account of the coral reef problem from the Darwinian point of view," brought together nonetheless a tremendous amount of very valuable factual information. Three years later J. Stanley Gardiner[17] published his *Coral Reefs and Atolls,* the volume being based on a course of lectures given in 1930 at the Lowell Institute at Boston. Although not documented in detail as was Davis' work, this volume shows a more objective approach and is probably the finest general presentation of the subject of existing reefs yet written.

In this interval T. Wayland Vaughan[46] wrote a great many papers about corals and coral reefs, based primarily on his studies of Tertiary and later structures in the Caribbean and Central America. Vaughan, like Gardiner, was familiar with reef organisms and their requirements. He recognized the necessity of studying each reef in relation to its local environment and was aware of the dangers of generalizations based too largely on physiographic form.

M. A. Howe[23] and, later, W. A. Setchell[37] called attention to the importance of algae in the building of "coral" reefs. A. G. Mayor[31] made detailed studies of the reefs of Samoa, later followed by R. T. Chamberlin.[3] H. A. Brouwer,[2] W. H. Hobbs,[20] P. Marshall,[29] G. A. F. Molengraaff,[32] H. Yabe,[48] and others studied the elevated reefs of parts of the Pacific. L. J. Chubb[5] published a number of papers giving the geological results of the St. George Scientific Expedition to many island groups in the Pacific in 1924–25, and Ph. H. Kuenen[24] as geologist on the Snellius Expedition in 1929–30 reported on many reefs in the East Indies. Hoffmeister[21] and Ladd[26] published the first accounts of their work in Tonga and Fiji.

During this period most workers had very definite ideas about reef origin and lent their support either to the subsidence group headed by Davis or to the glacial control group led by Daly. In his book on the coral-reef problem Davis lists thirty-five papers written by himself and a dozen published by Daly. Twenty years have passed since Davis published his volume, and Daly is still actively producing papers and books on the reef problems. These two indeed led the field in productivity.

THE PAST DECADE

During the first half of this period, which covered roughly the years of World War II, only a few papers appeared and these dealt mainly with prewar investigations. In 1939, prior to the outbreak of hostilities, J. H. F. Umbgrove[44] published an account of the atolls and barrier reefs of the Togian Islands in the Dutch East Indies. Clearly recognizing the importance of sedimentation in reef development, and unable to find any evidence to support glacial control, he described instead the evidence for crustal movements during the Pleistocene. He stated that the elevated reefs in the area were formed in late Tertiary or early Pleistocene time, then uplifted and eroded, the existing reefs developing later on a subsiding fault block. In areas where subsidence was slow, reefs grew to the surface, fringing reefs being transformed into barriers in true Darwinian fashion.

In the twenty-year period that separated World Wars I and II, the Japanese were in control of all of Micronesia, and foreign scientists had little opportunity to study in the area. The Japanese themselves carried on a great deal of work, but much of this was not published, or at least was not generally available until the end of World War II. One of the most active workers on Micronesian coral reefs—both the existing reefs and the elevated limestones—was Risaburo Tayama, of the Tohoku Imperial University at Sendai. A number of Tayama's preliminary papers have recently become available, but his final report on the character and distribution of coral reefs in the South Seas is still in manuscript form. It covers all his investigations in Micronesia from 1932 to 1943. In 1940–41 the Japanese issued a two-volume Jubilee Publication to commemorate the sixtieth birthday of Professor H. Yabe. Copies of these volumes were not available in this country until the end of the war in 1945. Volume II contained a description by Shoshiro Hanzawa[18] of cores from a deep drill hole on Kita-Daitô-Zima (North Borodino), a small island east of Okinawa. The hole was drilled in 1934–36 to a depth of 1,416 feet. (Reports on the drilling were published in Japanese by Rokuro Endo in 1935 and by Toshio Sugiyama in 1936.)

In 1922 H. C. Richards[34] gave a talk to the Royal Geographical Society of Australasia

(Queensland Branch), entitled "Problems of the Great Barrier Reef." This reef has long been recognized as the greatest of them all, extending as it does more than 1,000 miles off the northeast coast of Australia through 15 degrees of latitude and 11 degrees of longitude. Richards outlined a program to determine the origin, growth, and natural resources of the reef area. His recommendations led directly to the formation of the Great Barrier Reef Committee, a group of some sixty members representing leading scientific societies. The first six volumes of the Committee's reports were issued between 1925 and 1948 and contain many interesting data, particularly the results of drilling at two points on the reef.

The scientific director of the Great Barrier Reef Committee was Charles Hedley. His death in 1926 was a severe blow to the committee's program, particularly in the field of marine biology. Consultations with J. Stanley Gardiner led to the formation of an influential committee in England that organized the Great Barrier Reef Expedition of 1928–29, under the leadership of C. M. Yonge.[49] This group spent a year on the Great Barrier, and the results of their investigations are appearing in England in a valuable series of quarto volumes. These reports deal with physiology of reef organisms, the biochemistry of their environment, and the processes of erosion and sedimentation on reefs and low islands.

In 1943 T. Wayland Vaughan and John W. Wells[47] published a revision of the Scleractinia. This volume, though dealing primarily with the systematic classification of the stony hexacorals, included a summary of the distribution of reef-building types both geologically and geographically.

In 1944 Hoffmeister and Ladd[22] summarized the evidence for the antecedent-platform theory. This paper was the last of a series of three, the first two having dealt with the subsidence theory and the theory of glacial control. Their observations and conclusions were based almost entirely on field work done in Tonga and Fiji. Both of these island groups are inside the Melanesian continental area where, as most workers will agree, uplift rather than subsidence characterized late Tertiary and post-Tertiary time. There are thick sections of elevated limestones in both island groups that earlier workers had cited as evidence of subsidence. It was found, however, that these limestones represented several periods of deposition, and furthermore that many of them contained no reef corals or other evidence of deposition in the shallow waters of the reef-coral zone. As they found no sections of unquestioned elevated reefs thicker than 300 feet, these investigators concluded it was unnecessary to call on subsidence to explain the elevated reefs. Likewise, since many of the elevated reefs were found to be Tertiary in age, they could not accept glacial control as a general explanation, though they stated that changes of sea level had certainly stimulated the growth of reefs in Pleistocene times. In 1945 a full account of their work in Lau (eastern Fiji) was published.[27]

Since 1945 Hoffmeister and Ladd have studied both existing and elevated reefs in various parts of Micronesia and now feel that they were too restrictive in their identifications of "reef limestone." They failed to recognize that "reef structure" (imbricating colonies of flat reef corals in positions of growth such as characterize the marginal zones of existing reefs) forms a very small percentage of the entire reef. It is an important part, to be sure—as important as the sides of a pail that holds water—but it may make up only 5 or 10 percent of the reef mass, and furthermore may be the first part to be destroyed when the reef is elevated and eroded. By thus recognizing the quantitative unimportance of reef structure, the occurrence of scattered reef corals that are *not* in position of growth assumes greater significance. Such occurrences may indeed suggest a talus slope or deposition on a submarine bank, only small parts of which projected into the zone of reef growth, but it is perhaps more likely that such scattered corals were deposited on the wide reef flat behind the marginal zone or in the shallow waters of a lagoon.

A second factor overlooked by Ladd and Hoffmeister was the significance of the texture of sediments.[25] Fine sediments do not accumulate on unprotected banks, and, therefore, the occurrence of thick sections of such materials is evidence of the existence of a protecting rim at the time of accumulation, even though such a rim may no longer exist.

In 1945 Harold T. Stearns published a summary of the "Late Geologic History of the Pacific Basin,"[40] which was followed the next year by his "Integration of Coral-Reef Hypotheses."[42] Both of these are very stimulating papers that offer solutions for most of the problems of the Pacific, including those involving coral reefs.

Stearns expressed the belief that great eustatic shifts of sea level occurred during Pliocene and Pleistocene time as a result of changes in the configuration of the ocean basins. On a map accompanying each of the articles, he drew the "Sial Line" around the continental islands of the west-

ern Pacific (this line is a little east of the "Andesite Line" of earlier writers) and stated that during Tertiary and early Pleistocene time reefs developed on rising foundations (continental, or sialic, islands) to the west of this line, whereas to the east reefs developed on subsiding islands (oceanic, or simatic, islands). He also expressed the belief that only the glacial control theory could explain the vast majority of living reefs on both sides of the Sial Line.

One of the attractive features of Stearns' integration is his conclusion that the merits of the leading coral-reef hypotheses vary according to the geologic age of the reef and its location relative to regions of uplifting and subsidence. Such recognition gives to each hypothesis both a time and a place to operate. If Stearns' basic structural interpretation is correct, it offers an explanation for the puzzling regional relations in the Pacific, particularly the concentration of low islands in the simatic area. In describing reef development Stearns starts with the Pliocene and cites Andrews' conception of a great continental area creeping by undulations against a subsiding central Pacific block. According to Stearns, submarine folds were formed, and, as they rose toward the surface of the sea, reefs developed on their crests. He recognizes that many of the atolls are very broad—too broad to have been made by marine plantation during the glacial epochs of the Pleistocene. He feels that such wide atolls are crowns on broad anticlinal crests. This idea is graphically presented in a series of sections through banks and atolls. These diagrams, however, are greatly exaggerated vertically, and for that reason tend to be misleading. Kuenen[25] points out (p. 14) that the exaggeration obscures the fact that, whereas an atoll may be a few to many kilometers wide, the range in depth between the lagoon floor and the maximum depth for coral growth is very small, if not negative. Kuenen concludes: "One cannot assume a topographic high, as flat and horizontal as a billiard table to have been formed by diastrophic agents before the reef grew." If the explanation diagrammed by Stearns is to be applied to many atolls, a lot of narrow-crested folds and quite a number of broad ones would have to be stopped at critical levels very close to the surface—the banks within 600 feet of the surface, the potential atoll foundations within 300 feet. During a time of lowered Pleistocene sea level the banks would develop reefs while the atolls were being truncated; with a return of high level both types of structures would become atolls. These and the repetitive stages to follow in succeeding glacial stages call for the accumulation of coral talus slopes. This is a slow process, and, as Kuenen points out, there can hardly have been sufficient time for such accumulation during the low levels of the Pleistocene—at least not in the manner indicated by Stearns' diagrams.

It is perhaps unnecessary to point out that Stearns' diagrams, probably for the sake of brief presentation, greatly oversimplified conditions in the sialic part of the Pacific. In certain island groups the distribution of islands and atolls in broad arcs fits a fold pattern very well, but in other groups the arrangement is much more complicated.

In his paper on the integration of reef hypotheses Stearns refers to recent negative shifts of the sea, and states that most modern reefs are decadent as a result of these shifts and that many are practically devoid of living reef organisms. Stearns suggested, in a paper published some months earlier on a decadent coral reef on Eniwetok Island,[41] that a very late local change of sea level of a few feet in the Marshall Islands caused the decadence observed there. The stretch of reef specifically referred to is on the southeast side of the atoll, a section that was subjected to heavy bombardment a short time before Stearns visited it in June 1944. This bombardment and the spreading of fuel oil from ships were probably largely responsible for the condition noted by Stearns. When the same reef was examined by Tracey in June 1946, the reef was still dead, and much of the limestone was spongy and rotten from the action of boring organisms. Live corals and algae, however, were beginning to repopulate the marginal zone, and everywhere else on the atoll the reef was healthy, with algae and corals flourishing in the marginal zone. The reefs of nearby atolls (Bikini, Rongelap, Rongerik) were also examined by Ladd and Tracey and found to be healthy.

In 1946 H. H. Hess[19] described a series of seamounts—which he calls *guyots*—under the intriguing title "Drowned Ancient Islands of the Pacific Basin." He located about 160 flat-topped peaks between the Hawaiian Islands and the Marianas. These structures resemble truncated volcanic cones rising 9–12 thousand feet above the ocean floor, their tops lying 3–6 thousand feet below sea level. Some of the structures are isolated, but others adjoin atolls, two at Eniwetok in the Marshall Islands appearing to pass partly under the atoll in a curious relationship that suggests for the seamounts an age greater than that of the atoll. These structures are still but little known. Half a dozen short cores obtained by Emery from the seamount adjoining Bikini consisted only of

Globigerina sand. In 1947 the Navy and the Geological Survey flew a magnetometer over the seamount adjoining Bikini, finding a magnetic anomaly approximately twice the magnitude of that of Bikini itself.[1]

In 1946 Kuenen gave a series of special geological lectures at London University. The subject matter of two of these lectures was published the following year as a very interesting paper entitled "Two Problems of Marine Geology: Atolls and Canyons."[25] Kuenen is a skilled and experienced worker both in the field and in the laboratory. He was the geologist on the Snellius Expedition, where he gained firsthand information on reefs in parts of the Dutch East Indies. His full reports on the geology of the coral reefs[24] and his interpretation of the bathymetric results were published in 1933 and 1934. In his 1947 paper, leading coral-reef theories are critically reviewed, and a merger involving the two leading theories is proposed under the name "glacially controlled subsidence." Under glacial control he stressed solution of exposed preglacial reefs, concluding that by such means alone elevated rims were cut down partly or entirely to glacial sea level.

He believed that the proposed combination could meet the main objections raised against either subsidence or glacial control when applied separately, and that it accounted for some obscure points not previously stressed. Thus, denudation by chemical means yielded no smothering sediment and permitted reefs to flourish along the edges of platforms at glacial levels, and as a result limited postglacial reefs to the peripheries of such platforms. Kuenen argued for extremely slow preglacial subsidence as compared with rapid postglacial rise of sea level. He stated that very deep passages and deltas in front of gaps in atoll and barrier rims are rare or absent in coral-reef areas because preglacial subsidence was so slow that any gaps formed could be closed by coral growth. Postglacial rise of sea level, on the other hand, was held to be much faster, and hence many shallow gaps were left unfilled because the corals were not able to keep up with the rising waters.

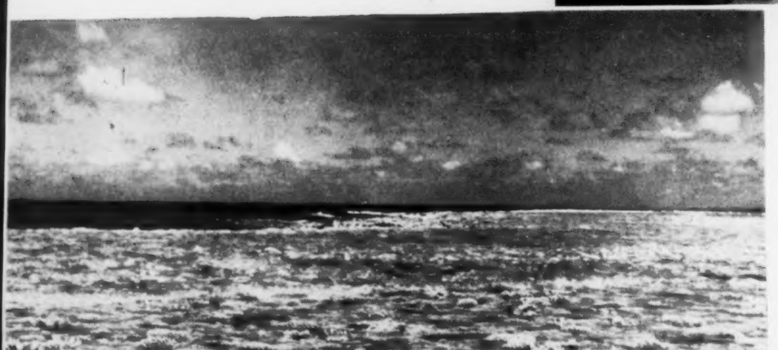
Geological evidence of widespread solution that is limited to the intertidal zone and particularly effective at low tide level, has been recently cited by Kuenen,[25] Umbgrove,[45] and Fairbridge and Teichert.[15] Solution phenomena had been observed by earlier workers, especially by Murray[33] and Gardiner,[17] who claimed that lagoons were hollowed out by this means, but oceanographers have pointed out that normal sea water is saturated with calcium carbonate and, therefore, incapable of dissolving limestone. Kuenen thought that solution might truncate large areas, but Daly questions this possibility.[7] Evidence showing, however, that water on reef flats varies considerably in its composition from that of normal sea water has been presented by biochemists working on the Great Barrier Reef[49] and at Bikini. The possibility of solution on a large scale as a result of this variation must be considered. In 1946 K. O. Emery[12] attributed the development of tide pools in calcareous sandstone at La Jolla to the diurnal variation in CO_2 of the pool water caused by the biologic processes of organisms inhabiting the basins. In the daytime, animal life uses up oxygen and respires carbon dioxide, whereas marine algae use up carbon dioxide and give off oxygen by photosynthesis, resulting in a marked increase of oxygen in solution. During the night, when photosynthesis does not operate, both forms of life give off carbon dioxide. At night, therefore, the increase of carbon dioxide in the water increases the amount of calcium ion that can be held in solution, and a dissolving of the rock walls of the basins would be expected. Conversely, during the daylight, the drop in carbon dioxide in solution, by this process as well as by the increased temperature of the basin water, results in deposition of calcium carbonate as a fine precipitate. As the precipitate would be flushed out by waves at high tide, the resultant of the daily cycle would be an enlargement of the basin by solution of the rock. Such a process is probably of great significance in the development of reefs, both because there it acts effectively over a broad flat that may be covered by only a few inches of water at low tide, and because of the abundance of organic life on the reef.

In 1947 J. H. F. Umbgrove[45] published a comprehensive paper reviewing all work on the coral reefs of the East Indies. He pointed to evidence for widespread subsidence and again expressed the opinion that the shifting of sea level in the Pleistocene was a factor of only minor importance in the development of East Indian reefs. He stressed the importance of sedimentation in lagoon filling and in the retardation of reef growth.

A brief report on the drilling done in 1947 during a resurvey of Bikini Atoll (Operation Crossroads) was published by Ladd, Tracey, and G. G. Lill[28] in 1948. In this paper it is noted that shallow-water assemblages were found at great depths, and the Bikini findings are compared with the results of drilling done on coral islands and reefs in other parts of the Pacific. It is clearly shown that we are not yet ready to generalize or to predict what the next deep hole on a coral

Part of an atoll reef. View looking north from Bikini Island, taken at about five hun feet. The island is about half a mile wide; to right is the open ocean, to the left the lago

Reef at Enyu Island, Bikini Atoll, showing fissures and open pools in the reef flat. The blowhole, fed by surf, is spouting water twenty-five feet into the air.



Coral growth on reef near Enirik Island, Bikini Atoll. Most of the flat is covered by only a few inches of water at low tide; the exposed coral colonies in the foreground are approximately a foot in diameter.

island will reveal. Thus, of the three deep holes drilled on islands in the open Pacific—on Kita-Daitô-Zima (a small island east of Okinawa), on Bikini, and on Funafuti in the Ellice Islands—the hole on Kita-Daitô-Zima was consolidated in its upper part, the one on Funafuti was consolidated in its lower part, and the one on Bikini, the deepest, was not consolidated at all. The Kita-Daitô section was dolomitized in its upper part, Funafuti in its lower part, Bikini not at all. The ages of the rocks penetrated likewise varied considerably.

Late in 1948 appeared the preliminary reports of Emery[14] and Tracey, Ladd, and Hoffmeister[43] on surveys carried out in 1946 in connection with Operation Crossroads. Emery, who was primarily responsible for the charting of lagoons and submarine slopes, showed clearly in the case of Eniwetok that the lagoon floors of atolls are not the smooth, saucerlike depressions many conceive them to be. When the 180,000 soundings obtained by the Navy in Eniwetok's 24-mile lagoon were plotted and contoured, they revealed terraces, depressions, and coral knolls of varying height. In this small lagoon more than 2,000 coral knolls were located.

Francis P. Shepard's[38] pioneering volume *Submarine Geology* appeared in 1948. It includes a chapter on coral reefs that is in effect a very good summary of the problem. He cites many of the most recent discoveries, relates the reefs to other submarine features, and wisely avoids the temptation to make final interpretations. In an article summarizing the evidence of world-wide submergence, published late in 1948, he concludes[39] that some of the indicated submergence was Pleistocene and some Tertiary or even earlier. His reason for extending the data into pre-Pleistocene time is largely the evidence from drilling on coral islands in the Pacific (Bikini and Kita-Daitô-Zima) and in the Atlantic (Bahamas).

The latest contribution to the resurgent coral-reef problem is one by Milton B. Dobrin, Beauregard Perkins, Jr., and Benjamin L. Snavely[11] describing geophysical work carried out on Bikini. In 1946 a seismic refraction survey was made by firing a series of depth charges on the lagoon bottom along four lines extending across the atoll, the resulting seismic waves being picked up by water-coupled microphones near shore. The time-distance curves indicated the existence of three zones of different sound velocity. The first of these was about 2,500 feet thick and showed a speed of 7,000 ft./sec.; the second extended to 10,000 feet with a speed of 11,000 ft./sec.; the third, a zone of undetermined thickness presumed to be the igneous basement, had a speed of 17,000 ft./sec. As a vertical velocity survey carried out in the deep hole drilled in 1947 showed a continuous transition from a velocity of 7,000 to 11,000 ft./sec. from the surface to 1,800 feet, the authors conclude that probably no essentially different rock materials appear in the atoll down to the foundation rock with the 17,000-ft./sec. velocity. The authors also state that under this interpretation the change in velocity in the upper layers would be due mainly to progressive compaction and cementation of calcareous sediments, but other possibilities involving a change in texture, dolomitization, or a change in type of material are mentioned. They point out that the seismic data call for relative subsidence in thousands of feet.

FUTURE INVESTIGATIONS

It should be apparent from the brief review here presented that there is a widespread and growing interest in reefs and in the oceanic environments in which they are developed. Some of the questions raised in earlier days have been answered, but as our knowledge of reefs has increased, new questions and promising new lines of investigation have appeared. With the help of improved methods and new techniques developed in the fields of submarine geology and geophysics, we may hope to settle some of the outstanding questions in the near future, and almost all of them eventually. Meanwhile, there are certain promising fields that may be pointed out; in most of these there is already some activity.

Island geology. Reef-encircled islands of all types should be mapped geologically, the maps being parts of an over-all survey in each island group. The U. S. Geological Survey is now engaged in such a program in Micronesia and the Ryukyus. Geological mapping is done on air photographs at scales of 1 : 10,000 or less, with a view to publication at a scale of 1 : 50,000; as a part of the study, reefs and lagoons are examined in some detail. Surveys of this type have recently been completed in Okinawa, Palau, Yap, and Saipan.

Areal studies should include detailed investigations of the effectiveness of solution: (1) on limestones above high tide; (2) on limestones exposed between tide levels; and (3) on limestones beneath the island but within the lenticular mass of fresh water (Ghyben-Herzberg lens) that, on the larger coral islands, extends to appreciable depths. There is evidence pointing to effective solution in all three of the above-mentioned en-

vironments, but supporting analyses are lacking, particularly for limestones below sea level. Quantitative data on existing conditions would be very valuable in appraising the importance of solution during the low levels of the Pleistocene.

Reef studies. Additional ecological studies such as those carried out on the Great Barrier[19] and at Bikini[35] on organic productivity on reefs are needed. These and related studies indicate the amount of organic matter produced, the amount available for burial in reef sediments, and set limits to the rate of reef growth under existing conditions.

Lagoon studies. Mapping and coring of lagoon sediments yield data that are of great value in the interpretation of elevated limestones and of the cores and cuttings obtained from drilling. When such information is combined with data obtained from dredging on the outer reef slopes, it is possible to make sound paleoecological interpretations and to determine the significance of the several types of limestone that occur in reef areas.

Recently completed detailed charts of a number of lagoons have revealed numerous coral knolls rising from the floor to varying levels, some of which are very close to existing sea level. A little has been learned about the surface features of knolls, but their internal constitution is unknown; no structures of this type have ever been drilled.

Submarine geology. Something of the abundance and distribution of seamounts in part of the Pacific is known, but, to date, as previously noted, very little else has been determined. Dredging and additional coring of these structures should give more clues to their origin and, perhaps, their age. Specifically, it is essential to learn if they have a hard rim with a central depression now filled with sediment; to determine if the coating of Recent sediments is thick, and if it contains pebbles of hard rock suggesting an origin by wave erosion.

Studies of isolated banks in the coral seas at 300–1,000 feet below sea level would give needed data on the types of sediments that accumulate and the kinds of organisms that live in reef areas at depths below the limit of reef-coral growth. Interesting comparisons could be made between accumulations on such banks and Tertiary sediments that are known to underlie reefs and coralliferous limestones on many Pacific islands. Emery[13] made such a study of the Ranger Bank that lies at 67 fathoms off the coast of Mexico, but in an area where reefs are sparingly developed.

Island foundations. The origin of coral reefs will probably never be settled to the satisfaction of many investigators until a great deal more is learned about the foundations of existing reefs. To date a considerable amount of information has been obtained from drilling and from geophysical investigations. The latter, of course, give only strong indications of the nature and distribution of the materials of the foundation, but these indications in themselves are quite valuable and such drilling as has so far been done bears out geophysical predictions fairly well. The seismic and air-borne magnetometer surveys in the northern Marshall Islands have already been referred to. Additional seismic studies are planned in that area, and it would be desirable to make some gravity studies. A pioneering effort in this field was carried out by the Japanese[30] on Jaluit Atoll in 1918. With little effort, a few measurements could be made in the lagoon and on the islands, but if such a survey is extended beyond the lagoon a submarine will be needed because of the steepness and the possible irregularities of the reef slopes.

One or more holes drilled through a coral atoll will certainly be required for a final solution of the reef problem. Plans for such a project have been made and will be carried out if financial support can be obtained to supplement that already offered by the Geological Society of America and the Office of Naval Research. Bikini has been selected for the proposed operation because at the present time more is known about that atoll than about any other. Geophysical data indicate that the hole would have to be 7,000–10,000 feet deep (depending on the site chosen). The operation would give valuable information on many fundamental geologic problems other than the origin of the atoll itself. Among these are stratigraphic, paleontologic, and paleoecologic data on an unknown geologic section; siesmic velocities and other geophysical information needed to interpret surface surveys; data on the Tertiary and earlier history of the Pacific Basin; information on the postdepositional alteration of carbonate sediments by compaction, cementation, recrystallization, dolomitization, and by differential solution; and data on the effects of the above-mentioned diagenetic processes on porosity and permeability of limestone reef rock. Much of this information would be useful not only in studies of other existing reefs, but also in studies of ancient reefs, including the oil-bearing structures of the Paleozoic.

After the present article was sent to press, the writers learned of a brief review of coral-reef theories published in New Zealand (COTTON, C. A. The Present-Day Status of Coral-Reef Theories. *New Zealand Sci. Rev.*, 1948, 6, (6) 111–13.).

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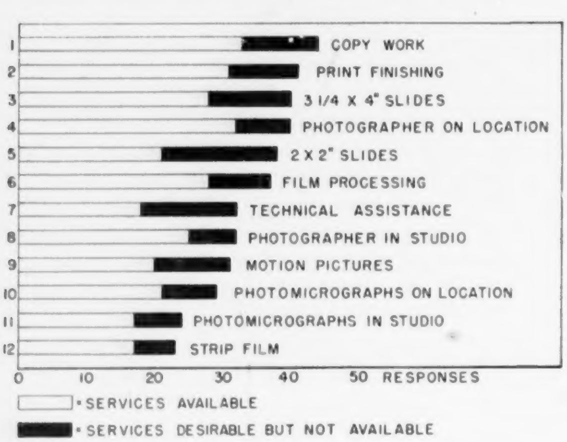
PHOTOGRAPHY AS A BASIC RESEARCH TOOL*

BANNER BILL MORGAN and DEAM HUNTER FERRIS

Dr. Morgan (Ph.D., Wisconsin, 1941) is professor of parasitology in the Department of Veterinary Science of the University of Wisconsin, where he has taught since 1941. Mr. Ferris was instructor in photography at Wisconsin during 1946–47 and is at present director of audio-visual aids and instructor in biology at Graceland College.

AT THE time the Annual International Photography-in-Science Salon was being organized by THE SCIENTIFIC MONTHLY and the Smithsonian Institution, Morgan and Ferris were conducting a survey to determine the extent to which colleges, universities, and commercial research laboratories were providing adequate photographic facilities for their scientists. The following situation appears to be typical in most institutions:

There are a confusion of small darkrooms on the campus and a hard-working but poorly equipped, understaffed, and inadequately housed central photographic laboratory. Various departments, dissatisfied with the services, build more darkrooms, which add to the confusion. Often the more vocal groups on the campus utilize a large share of the photographic department's time; since their photographic budgets are larger, an organization pursuant to these needs is built up. Thus, a vicious circle is established in which the scientist is driven more and more to meet his own needs, to the detriment of real service, economy, and efficiency.



Availability of photographic services in 48 educational institutions.

* All photographs from the Annual International Photography-in-Science Salons, sponsored by THE SCIENTIFIC MONTHLY and the Smithsonian Institution.

Our survey was designed to determine what situations were confronting the scientist that limited his use of photography in his research problems. Seventy-five questionnaires were sent out to scientists in the biological, medical, and veterinary fields. Fifty-three responded, representing 48 institutions in 28 states, Hawaii, and 2 provinces of Canada.

The survey was not intended to give exact statistical answers; owing to the complexity of the problem and the individuality of the scientists and laboratories represented, this would be difficult. It is believed, however, that a general picture of methods used by scientists for photographic services has been obtained.

How often are photographs or motion pictures made for scientific purposes and at what intervals? For what purposes are the pictures utilized? Are professional services available and at reasonable prices? What type of photographic organization serves the scientific laboratory? With what equipment are scientists doing their own photographic work? How much money was available for photographic budgets? How well are scientists satisfied with present services? What type of organization do they consider best for their needs? These and other questions were covered by the survey.

Seven laboratories reported the availability of special techniques or equipment for photography as follows: Phase contrast microscope, 1; electron microscope, 2; radioactive tracer photography, 1; motion photomicrography, 3.

Fewer than two thirds of the institutions reporting made available to scientists even the most basic photographic assistance, such as film processing, print finishing, or a photographer in a studio. Fewer than half furnished photomicrography, motion-picture photography, or projection-slide facilities. Only a third provided any technical assistance.

From the results of the survey it is clear that the demand for photographic services by scientists is

great, and that these demands are not now being adequately met.

FREQUENCY OF DEMAND FOR PHOTOGRAPHIC SERVICES

A summarization of the replies showing the frequency of the demand for photographic services by scientists is as follows: a few each year, 10 percent; at various intervals, 39 percent; every week, 25 percent; daily, 25 percent; no answer, 1 percent. All the services that were in demand are shown in the diagram. The production of $2'' \times 2''$ slides, technical assistance, photomicrographs to be taken on location, $3\frac{1}{4}'' \times 4''$ slide production, copy work, and motion-picture photography, in that order, were considered most desirable among the unavailable services.

Many laboratory directors who believe that photographs are not within their means always have a few taken each year for the annual report or to illustrate papers. This is possibly reflected in the response of those using pictures at irregular intervals and a few each year. The high esteem placed upon the photograph as objective evidence is shown by the number indicating such services were desirable but unavailable.

Although rigid conclusions are difficult to draw, certain trends may be noted. It is obvious that the great majority of those surveyed appreciated the value of photographs. Approximately 50 percent took photographs each week, and of this group 50 percent took some pictures every day.

Many scientists were forced either by inclination or in desperation to do their own photographic work. The scientist in his own laboratory, without the help of a professional photographer, may do all the work (29 percent); more than half (21 percent); less than half (16 percent); none (33 percent).

PURPOSES FOR WHICH PHOTOGRAPHS WERE TAKEN

The purposes for which the pictures were taken revealed some interesting information: illustrations, 88 percent; visual aids, 77 percent; research records, 65 percent; research data, 52 percent. Of those surveyed, 38 percent used all four types; nearly all indicated at least two uses. Apparently most scientists try to have pictures taken by professional photographers if they are available at the institution, as shown by the following figures (percentages indicate the proportion of scientists using such services): commercial, 7 percent; departmental laboratory, 10 percent; central laboratory, 51 percent; none, 32 percent.

As might be expected, the traditional use of photographs as illustrations in books and other publications ranked highest, 88 percent using them for this purpose. About 77 percent used photographs for visual aids in teaching. Of the services most desired and now available, "copy work" ranked highest; of the services most desired, but unavailable, production of $2'' \times 2''$ slides ranked first. Certainly, institutions that do not offer these services need to reconsider the value of their present photographic organizations.

Although the number using pictorial methods for records and to obtain data was not as great, these figures indicated a strong interest. In this area lies much of the opportunity for future growth and development. Photography holds the answer to problems of greater accuracy, timesaving, and undisclosed truth from data obtained by high-speed photography, time-lapse motion-picture studies, and similar methods that remain at present largely untried. As yet, satisfactory methods for the routine use of such photography are somewhat undeveloped.

Cattle heel fly (*Hypoderma lineatum*) depositing eggs. (Banner Bill Morgan and Deam Hunter Ferris.)

In an effort to determine the value of photomicrography to implement and even supplement conventional drawings, trials were conducted in our laboratory during the summer of 1947. We were engaged in making autopsies of various animals in order to collect parasites for ecological and incidence studies. Descriptions and detailed morphology are most important in studies of this nature, for purposes of identification and because of

To study mechanism of detergent entering solution, a tergitol droplet was allowed to fall into a transparent cell of water in a slide projector. (D. C. Whitmarsh, Ordnance Research Laboratory, Pennsylvania State College.)

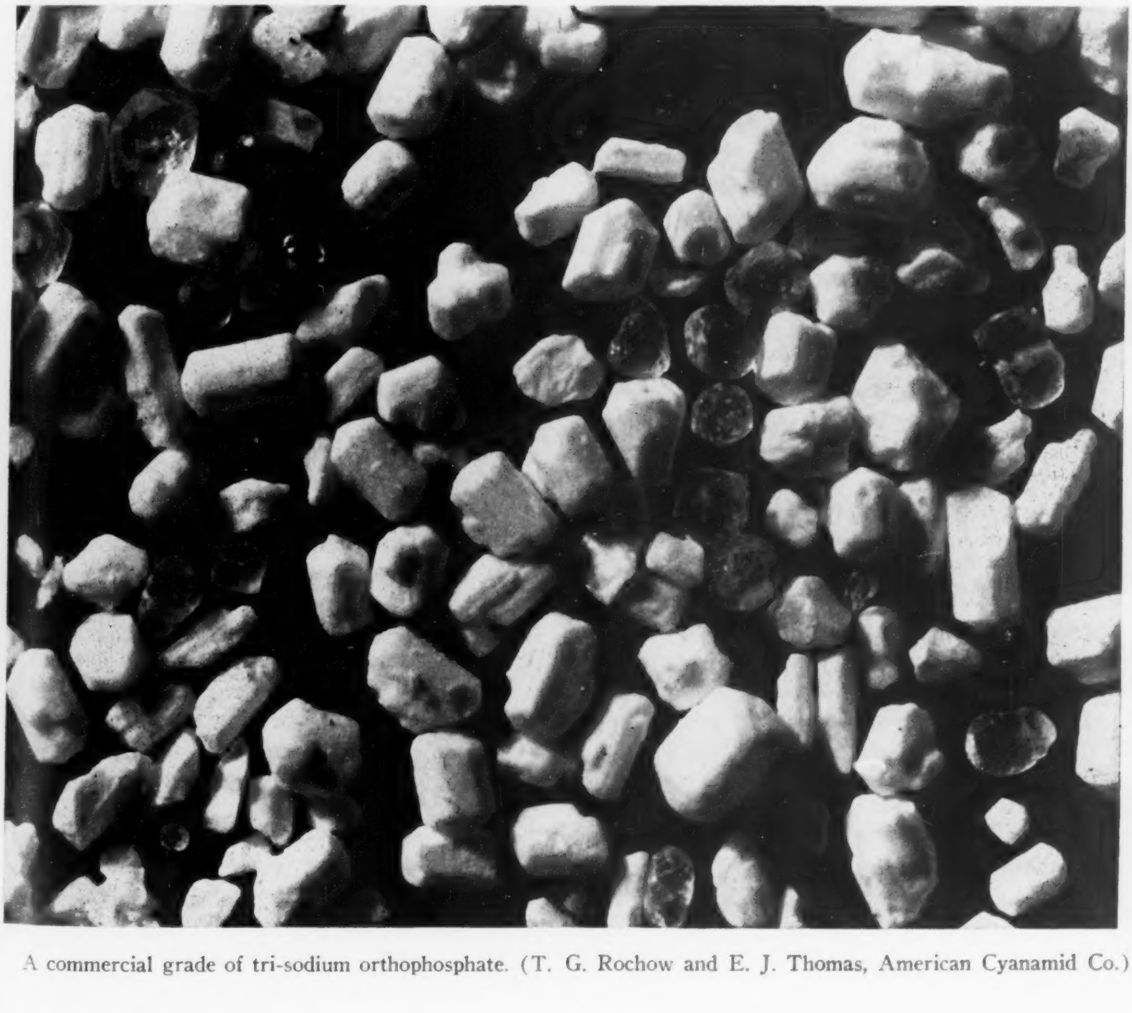
the ever-present possibility of new species being encountered. Since it is desirable to make some observations and measurements on living specimens, this created a real time problem. It was not always possible to delay "posting" the animals long enough to make detailed drawings. An Argus C-3 and a Leica camera, each equipped with copy attachments and extension tubes, were set up on an improvised copy stand in such a manner that they could be dropped over the microscope and a photograph taken in a matter of seconds. Considering only its minimum value as a method of obtaining accurate measurements, this was found to be far more rapid than making sketches and measure-

ments. It was more rapid than even the camera lucida. An ocular micrometer used to superimpose a scale upon the figure made a permanent record. In addition, it was found that some detail, especially in flattened specimens, was quite satisfactory; however, the inherent problems of depth of field make it improbable that photographs will ever supplant drawings completely. Clarity and freedom from distracting detail make drawings essential.

There is the possibility that the phase contrast microscope will help in the recording of data. Living, unstained tissues are revealed with all the detail of stained specimens where the material is suitable and appropriate instrument and technique are combined. With this microscope it is unnecessary to "stop down" the condenser diaphragm to obtain greater detail, making possible use of the objective at its greatest resolution. With proper photomicrographic equipment it will be possible to photograph and study living material at the same time. The inclusion of a Speedlamp in the illuminating system would easily allow photographs to be taken of the most rapidly moving specimen.

Such methods, where they would fit the research, are now partly available at reasonable cost. The inexpensiveness of 35-mm film should offer opportunities for data and record work. Even in these times of high prices, bulk 35-mm film was obtained at one-half cent per foot for "movie ends" (usually very satisfactory) or five to eight cents a foot for the very best quality bulk film sold exclusively for miniature use. Equipment is now available to convert 35-mm cameras to such uses as photomicrography, copying, and microfilming.

In the field of visual aids, the 2″ × 2″, 35-mm slide, and strip film are rapidly replacing the larger 3¼″ × 4″ slide. Especially for color slides, this



A commercial grade of tri-sodium orthophosphate. (T. G. Rochow and E. J. Thomas, American Cyanamid Co.)

results in economy and space saving. It should be mentioned that the larger film sizes yield better detail and are much superior for illustrative purposes.

Nearly 40 percent of the scientists surveyed used photographs for all four purposes (records, illustrations, data, and visual aids). This showed that a large proportion of scientists are alert to the possibilities of photography and that we should expect increased use in the future.

USE OF PROFESSIONAL SERVICES

That only 7 percent of the responding scientists used the services of commercial photographers indicated how far the average professional photographer is from the field. Scientific photography not only requires special photographic knowledge, but also presupposes some scientific background; often a knowledge of microscopy and related techniques is essential. From the standpoint of the commercial photographer the field is restricted. In large centers the specialist in medicine will no doubt have an excellent though rather limited opportunity.

Only 10 percent made use of or had available departmental photographic laboratories. From the standpoint of the department as a unit this is often a very satisfactory method of organization. In the case of a large department, such as a hospital, medical school, or research unit with a volume of photographic work, it is a sound system from the standpoint of efficiency and economy. In some cases the type of research work done makes this imperative.

The fact that more than 50 percent had central laboratories available would indicate this is the preferred method of organization. Although nearly all had photographic equipment (76 percent), 32 percent did no photographic work of their own. About 84 percent of this group had a central laboratory available. Many took color photographs and simply mailed the film to the processor.

About 29 percent did all their own work; only 26 percent of this group had the use of a central laboratory. It is obvious that a satisfactory central laboratory will be used. The large percentage of scientists doing most of or all their own work (53 percent) and the frequent expressions of dissatisfaction with the present system (but accompanied by a desire for professional photographic services) should give administrators and photographic directors some food for thought. Scientists want good photographic services, but our survey indicated that as a rule these services are not provided.

BUDGETARY ALLOWANCES FOR PHOTOGRAPHY

Nearly all replies indicated the need for larger photographic budgets. Photography is often regarded as a luxury. Many people think of it largely in connection with vacation trips or a Sunday afternoon excursion with the Baby Brownie. Most of the scientists used photographs, yet only 57 percent had photographic materials furnished in an unqualified way as a part of departmental supplies. Many scientists purchased their own supplies, yet only 21 percent were able to obtain supplies at a discount—which is certainly the least that could be expected!

Amounts available to scientists for photographic work were somewhat discouraging. Only a few more than half revealed any information regarding their annual budget, some giving reasons such as bookkeeping problems, or that such information was regarded as confidential. Twenty-three replies indicated a distribution of from \$50 to \$1,000, which was perfectly reasonable and surprisingly uniform. The next two jumped to \$5,000 and \$6,000. Two laboratories in commercial pharmaceutical houses spent as much for photography as one entire school. It would be noted that university budgets include many nonscientific uses for photographs. Surely the disparity here is glaringly apparent.

NEED FOR IMPROVEMENT OF PHOTOGRAPHIC LABORATORIES

A large majority of the replies showed dissatisfaction with the present photographic organization. A central laboratory, under the direction of an alert, competent director, and well equipped to do scientific work, is the most desirable. The mere existence of a central laboratory, however, is no assurance that scientists will receive adequate service; in fact, a large number of complaints were received from many who have access to such facilities. Although about 29 percent of the scientists did all their own work, even those who had a central laboratory, this was not entirely from choice, as shown by the number who checked the services they desired, and from statements of dissatisfaction. A few of these were: "Service slow but adequate;" "Central lab too busy;" "Not adequate because not immediately available; available on interdepartmental order [in other words, too much red tape];" "Service almost useless;" "Photographer, although good, is rushed and no train-

ing in scientific work;" "Professional photographers are interested in the artistry of photography and do not have a scientific or illustrative viewpoint." (This criticism has been heard repeatedly.)

Sound, color, time-lapse study, or high-speed motion pictures will be beyond the reach of the small laboratory. With a strong central organization, such projects could well be provided for most scientists because of the increased personnel strength and more efficient use of funds, equipment, and staff. With a large organization the time of its members can be utilized to better advantage and opportunity afforded men to specialize, permitting a much higher standard of services. Labor-saving equipment and competent assistants should free the photographer from routine duties and work requiring little skill. The smaller laboratories will find the use of mechanical washers, print dryers, and other labor-saving equipment profitable. A good central organization should have equipment to lend and rent. Portable photomicrographic equipment, especially, should be available.

Photomicrographs on location are much in demand and should be provided for. Preparations are more easily made in the scientist's own laboratory where he has all the accessories required at hand. Scientists experience difficulty in using equipment at the studio. It is often difficult to find fields on the large photomicrographic cameras of the horizontal type. Properly set up and equipped, the scientific photographic unit should provide equipment where the scientist can sit down and find the microscopic fields as comfortably as in his own laboratory. Often the scientist will be unable to use the type of illumination necessary for photomicrographs.

The photographic department of a large institution should be given some opportunity for research. This could possibly be carried out in conjunction with the physics, chemistry, and other departments, as well as the larger photographic companies, who have excellent research departments but who may find it more profitable to concentrate on problems other than those of the scientist. It is hoped that in the future scientists will use photography more and more in their research as they become aware of the ways in which photography can improve their work.

MOUNTAINS

A sphere in space
At sun-up's stretching time
In satisfaction belched
And squeezed some splinters
From its skin,
Then went about its business.

Thus came the orphaned ones—
To face the floggings of the wind,
The water's acid burns,
The searing etching of the ice,
The senseless scrubbing of the sands,
The torments of the damned.
Yet, they stand,
As when they first were fire-forged,
And mark the graves
Of pampered and of weaker ones.

JOSEPH HIRSH

THIRD ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON*

Prize winners in the Third International Photography-in-Science Salon, an annual competition for scientists and photographers, sponsored by THE SCIENTIFIC MONTHLY and the Smithsonian Institution, were announced on September 25.

Judges were Merle A. Tuve, of the Carnegie Institution, for the physical sciences; Walter F. Jeffers, of the Department of Botany, University of Maryland, for the biological sciences; A. A. Teeter, of Charles Pfizer & Co., New York City, for chemistry; Emanuel Krimsky, of Polyclinic Hospital, New York City, for the medical sciences; and Alexander J. Wedderburn, of the Graphic Arts Division, Smithonsian Institution, for photography.

The 200 prints were on exhibition at the U. S. National Museum, October 3–31, and will be shown at the Annual Meeting of the AAAS, New York City, December 26–31. Afterwards the show will go on tour of important museums and scientific institutions in this country and abroad.

Prize winners in the Black-and-White Division:

First: L. L. MARTON, Chief, Electron Physics Section, National Bureau of Standards, Washington, D. C.: "Electron-optical shadow method."

Second: BERNARD HENRY MOLLBERG, University of Houston, Houston, Texas: "Ventrosinistral view of dried chick embryo, plated with aluminum."

Third: S. B. NEWMAN, EMIL BORYSKO, and MAX SWERDLOW, National Bureau of Standards, Washington, D. C.: "Electron micrograph of thin section of cells in onion root tip."

Honorable Mention: JOSÉ OITICICA, JR., Rio de Janeiro, Brazil (Guggenheim Fellow at the U. S. National Museum, Washington, D. C.): "Male genitalia, ventral view, of *Citheronia mogya* Schaus 1920 (Lepidoptera, Citheroniinae).

CLYDE T. HOLLIDAY, Applied Physics Laboratory, The Johns Hopkins University, Silver Spring, Maryland: "Cloud formations." (Photograph made from a V-2 rocket at White Sands.)

T. G. ROCHOW, American Cyanamid Company, Stamford, Connecticut: "Commerical sample, tri-sodium orthophosphate ('TSP')."

CHARLES J. SALAT, Armour Research Foundation, Chicago, Illinois: "Calibration of a ball bearing by means of optical flats."

CLEE O. WORDEN, Laboratory of C. A. Zapffe, Baltimore, Maryland: "Fractograph of piezoelectric single crystal."

In the Color Division, the following won awards:

First: CHARLES D. OUGHTON and EUGENE C. RICKER, Battelle Memorial Institute, Columbus, Ohio: "Xerographic developing process."

Second: CHESTER F. REATHER, Carnegie Institution, Baltimore, Maryland: "Implantation of twelve-day human ovum."

Third: THOMAS C. POULTER (third-time prize winner) and WALTER LAWTON, Stanford Research Institute, Stanford, California: "High-speed movies of colored meteorological balloons used with Poulter Seismic Method show interaction of shock waves in the third dimension."

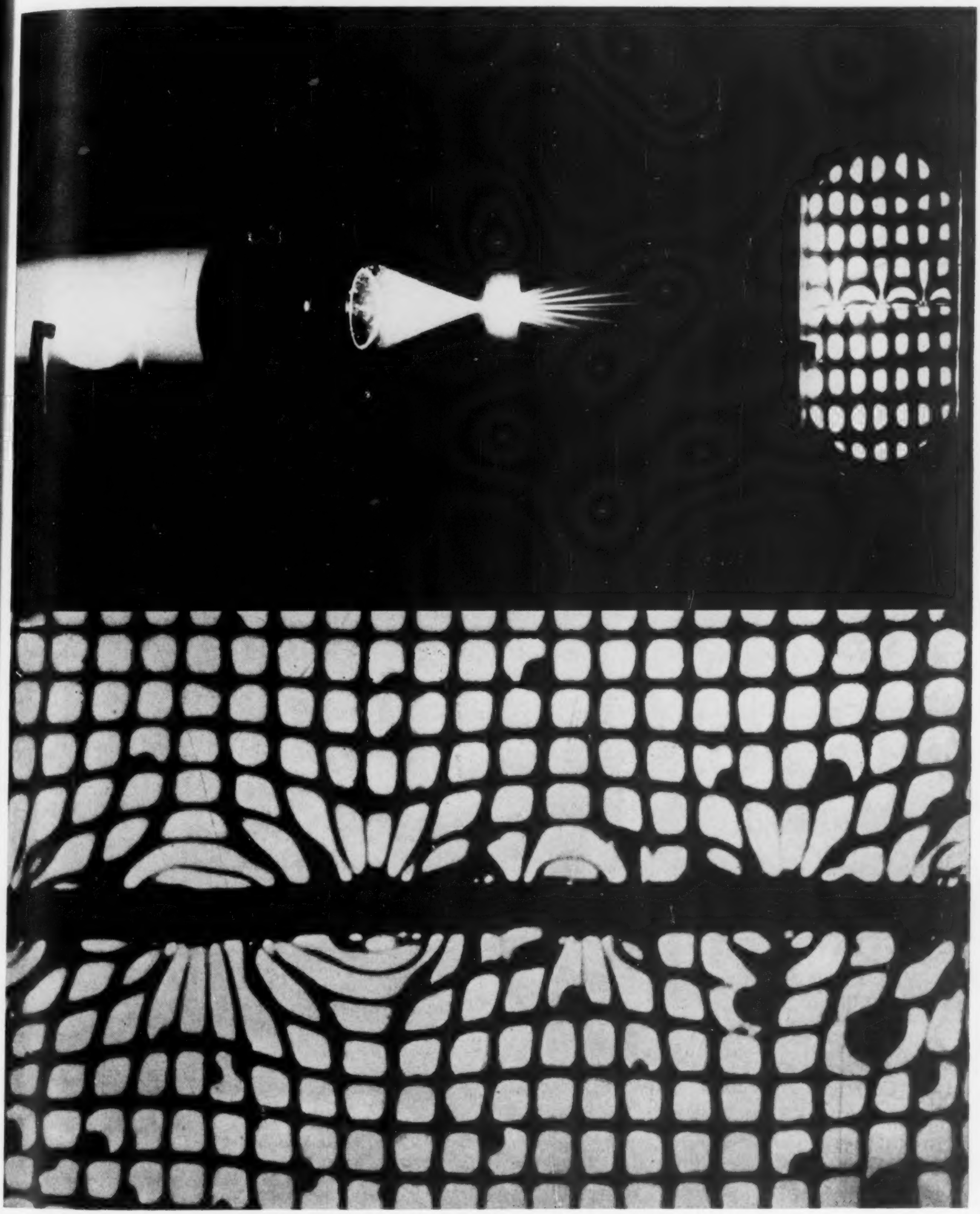
Honorable Mention: D. H. ROWLAND, Carnegie-Illinois Steel Corporation Research Laboratory, Pittsburgh, Pennsylvania: "Polished and etched cross section of experimental galvanized coating on low carbon steel."

ROWLAND B. STRADLING, U. S. Cast Iron Pipe and Foundry Co., East Burlington, New Jersey: "Photomicrograph at 200 diameters of titanium nitride in cast iron."

ALBERT C. WALKER, Bell Telephone Laboratories, Murray Hill, New Jersey: "Quartz crystal grown at Bell Telephone Laboratories."

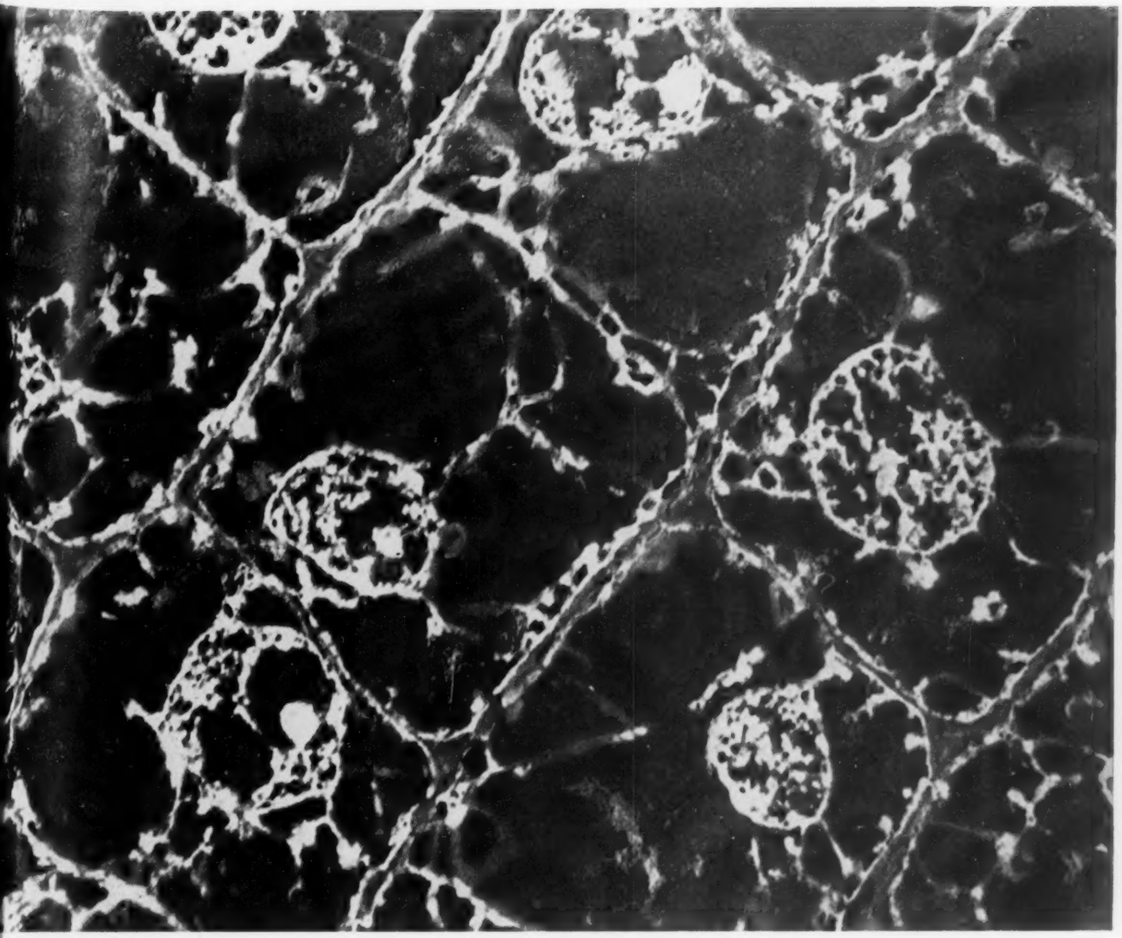
Established to encourage and extend the use of photography as a basic research tool, the contest will be continued in 1950. All entrants shall be actively engaged in scientific research, and all photographs must be taken for scientific purposes. Entries may be sent to the Editor, The Scientific Monthly, 1515 Massachusetts Ave., N. W., Washington 5, D. C., November 1–27, 1950. Prize-winning and other accepted entries will be shown at the Annual Meeting of the AAAS in Cleveland, Ohio, December 26–31, and at the U. S. National Museum, Washington, D. C., January 3–31, 1951.

* Prints in the 1949 Salon are booked through April 1950. Dates for showing these pictures may be arranged by writing to the Editor, THE SCIENTIFIC MONTHLY.



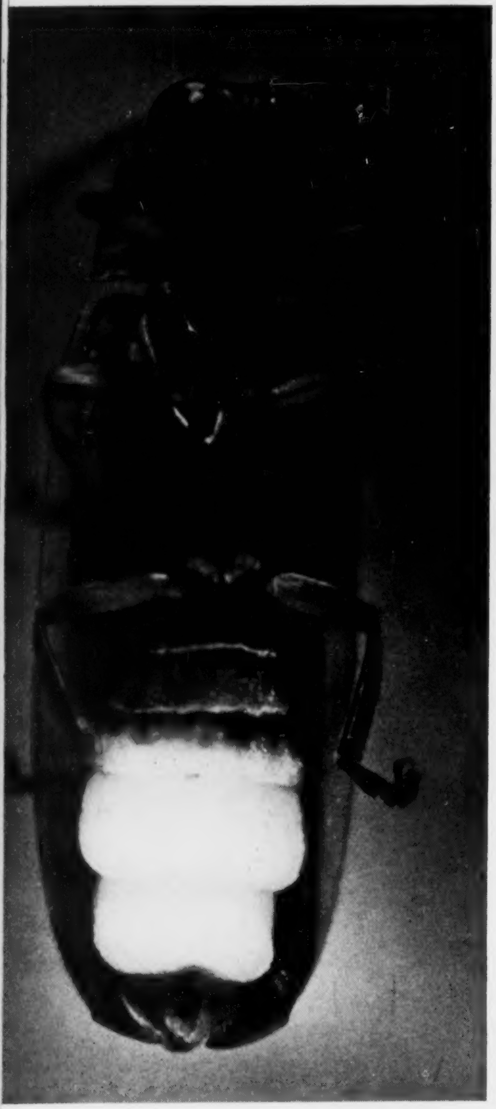
First Prize went to L. L. Marton, chief, Electron Physics Section, National Bureau of Standards, for this illustration of the new electron-optical shadow technique, which makes it possible to photograph and study quantitatively electrostatic and magnetic fields of extremely small dimensions. *Above:* An analogous experiment in light optics. In the electron-optical shadow method, the glass lens system is replaced by an electron lens, and the distorted plastic by a magnetic or electric field. *Below:* Photograph of a typical pattern. Superposed on image of a magnetic recording wire is the electron shadow of a fine wire mesh placed just beyond the back focus of an electron lens. From the displacement and reduced magnification of a selected part of the mesh, the absolute value of the magnetic field intensity at a corresponding point in the field can be accurately computed.

Photomicrograph showing orientation structure of an annealed 70–30 nickel-copper alloy that had been deformed at room temperature. At the National Bureau of Standards it was found that the proper etching technique will produce an optically active film, which, when photographed under polarized light, will show variations in orientation. Print entered by D. H. Woodard, metallurgist, National Bureau of Standards.



▲ Electronmicrograph of an extremely thin cross section of onion root tip in the zone of elongation, showing cell walls and nuclei. Prepared for study by a new method in ultramicrotomy recently developed at the National Bureau of Standards by S. B. Newman, Emil Borysko, and Max Swerdlow. Awarded third prize.

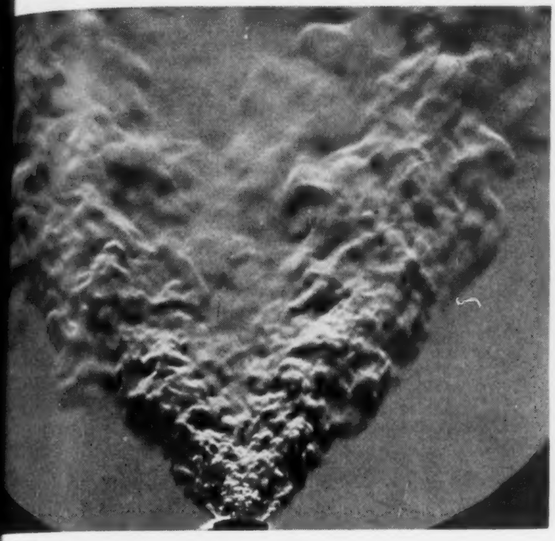
Grenz ray picture of a rose. Part of an investigation to study plant nutrient movements. Entry from Herbert R. Isenburger, St. John X-Ray Laboratory, Califon, N. J. ▶



▲
José Oiticica, Jr., of the Museu Nacional, Rio de Janeiro, temporarily at the National Museum, Washington, D. C., under a Guggenheim Fellowship, took this ventral view of the male genitalia of *Citheronia mogya* Schaus 1920 (Lepidoptera, Citheroniinae), which won Honorable Mention in the Black-and-White Division.

▲
Among the interesting zoological subjects was this photograph of a firefly taken by its own flash. Submitted by A. M. Winchester, of Stetson University.

This ventrosinistral view of a dried chick embryo which has been plated with aluminum won Second Prize for Bernard Henry Mollberg, of the University of Houston. The plating process reveals details of surface anatomy that would otherwise remain obscure. ▶



Schlieren photograph of helium discharging from injector of a V-2 rocket motor. Print entered by Walter R. Keagy, Jr., of Battelle Memorial Institute.

Calibration of a ball bearing by means of optical flats. Photograph provides permanent record of the calibration of a half-inch ball bearing. Honorable Mention awarded to Charles J. Salat, of Armour Research Foundation.

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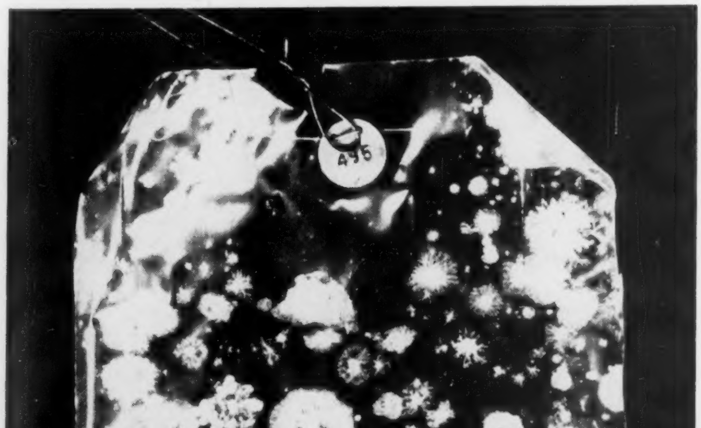
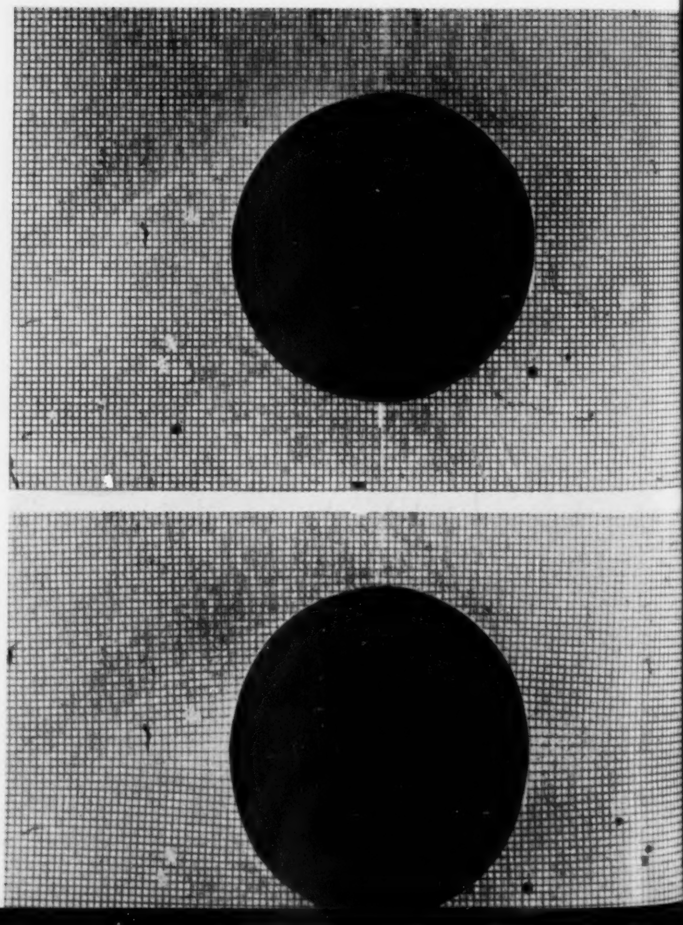


Mold growth on cellophane exposed in the tropics. Print entered by Leonard Teitell and Marie Reeder, of Frankford Arsenal's Pitman-Dunn Laboratory.

◀ Honorable mention went to Clee O. Worden, of the Laboratory of C. A. Zapffe, research metallurgist, Baltimore, for this fractograph of a piezoelectric single crystal. "Fractography" is a photographic technique that surmounts the long-standing difficulties of observing jagged surfaces at high magnification.

By means of a new photo-grid technique ▶ developed at the National Bureau of Standards, plastic deformations in the vicinity of holes may be conveniently investigated. Method involves use of a master grid ruled with lines 0.015 mm wide and nominally spaced 0.25 mm apart. Submitted by James A. Miller, metallurgist, Chester I. Pope, chemist, and Benjamin L. Page, physicist.



SAMOA—SHELL-COLLECTOR'S PARADISE

R. TUCKER ABBOTT

Mr. Abbott was malacologist on the Harvard-Archbold Expedition to Melanesia in 1940–41, collecting mollusks in Fiji for three months aboard the junk-yacht Cheng-Ho. *In World War II he saw service as a Navy dive-bomber pilot and later as malacologist for Navy Medical Research Unit No. 2, based on Guam. He has done research on disease-carrying snails in China and the Philippines, and since 1946 has been assistant curator of mollusks at the U. S. National Museum, Washington, D. C.*

AMONG the many Polynesian collecting grounds for mollusks, Tutuila Island, Samoa, stands foremost as a rich and diversified area for ecological studies. The coastal barrier reefs present ideal collecting conditions, but it is no less rewarding to search for sea shells along the rocky shores, on the sandy beaches and mud flats, and in the mangrove swamps. The beach collector soon finds that certain groups of marine animals inhabit special areas that suit the needs or desires of the individuals. Fagaitua Bay, on the southeast coast of Tutuila, is as varied in its types of habitats as New York City is in its wards. From the shallow mud flats to the rock-bound headlands, from the semistagnant waters of the mangroves to the turbulent surf on the reefs, every kind of footing and resting place is available for the hundreds of species of marine shells found in this region.

Frequent visits to Fagaitua Bay enabled me to observe the quite obvious differences between the way of life on the sand flats and the rocky shores, and also to see a constant change in each one of the littoral environments. In fact, I found it impossible, for instance, to make a detailed census of the shell population on the sand flat just inside the reef barrier, for on each visit I found the sand bar had been moved by the tides and currents. One section of the reef bordering the lagoon would be covered by sand one day and bare the next. Some changes in the bay were slight and occurred at regular intervals, such as the tidal flooding and draining of the mud flats; other changes were more violent but less frequent, as for instance when the elements played rough during stormy moods. After a heavy gale the deep, soft sand of a beach would be replaced by a mass of coral rubble, and sometimes sections of the inner coral reef would be half covered or isolated by a miniature Sahara.

In a week or so of more normal weather—gentle northeast breezes and a calm sea—the lagoon would slowly change back to its original condition. Inch by inch the sands would move back to the beach, and eventually the reef would recover its old boundaries. All these changes the mollusks of the bay had to endure. A few succumbed in exceptionally long periods of change, as was evident from the dead and empty shells cast up on the beach. In the main, however, the populations survived the ordeals which, to us, would have been equivalent to violent earthquakes, devastating floods, and dust-bowl storms.

It was not until my fourth or fifth visit to Fagaitua Bay that I became acquainted with a molluscan sand lover. Directly in front of Alofao Village is a square half mile of sandy flats. For the Tom Thumb conch shell, *Strombus gibberulus* Linné, these flats are paradise. My West Indian collecting had long made me class the conch shells among the Gargantuas of the shell world. The pink-and-ivory flushed queen conch of Florida and the Antilles, *S. gigas* Linné, grows in most cases to a foot in length. Here on the Fagaitua flats the adults of this diminutive conch were no more than an inch and a half in length. I was as much surprised as if I had found an adult elephant the size of a dog.

The first few times I searched the flats, I had seen neither the Tom Thumb conchs nor any trails in the sand that might have indicated their presence. Then one afternoon at low tide, as I walked out from the beach, I discovered them by the thousands. For the next week I raided the colony for specimens, without seemingly reducing their numbers, until one day I came down to the flats to discover every single conch had disappeared. Twice again, during my occasional visits for the next three months, the Tom Thumbs appeared for a few days to enjoy what apparently was a lengthy period of mating. Each time they moved off as mysteriously as they had come. I was not long

enough in this vicinity to find out whether this appearance was a quarterly or even monthly occurrence. Nor was I able to ascertain positively that the conchs had left for deeper waters. Perhaps they had retreated to a less active life among the corals of the reef or had dug themselves into the sand at the bottom of the lagoon.

My tiny conch animal may have been of Lilliputian proportions, but he certainly made up for it in strength and agility. The chitinous trap door, or operculum, has been modified into a long, sickle-shaped weapon. Most mollusks use this operculum as a door to slam shut in the face of the enemy, but Tom Thumb uses it as cutlass and vaulting pole. The small, saw-toothed operculum is attached to the end of the powerful foot, where it can be brandished effectively against attacking crabs. The foot muscles are powerful enough to force open a boy's fingers when the shell is held in his clenched fist. A rock ten times a conch's own weight can be pushed aside with ease.

My respect for Tom Thumb increased even more when I saw him try out for the pole-vaulting record. Apparently bored at times, just inching along on the sand flat, the conch whips up his operculum, jabs it into the sand, and suddenly hurtles himself a full two inches off the ground. If a one-legged man with a crutch could jump twelve feet into the air, it would be no less amazing.

The eyes of Tom Thumb are something marvelous to behold! In a glass jar of sea water, the tiny conch loses all shyness, and soon comes out of his shell. At first, two round orbs peek out between the half-extended foot and the shell—two shiny agates marked with black and orange circles. Having those eyes peering directly at you tempts you to believe that high intelligence is in the brain they serve. Presently the bulbous eyes slide for-

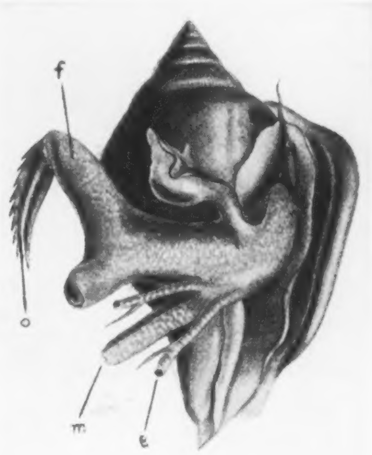
The giant and the dwarfs. A study in contrast between the diminutive conchs, *Strombus gibberulus* Linné, and the massive helmet shell, *Cassis tuberosa* Linné.

A Samoan bay, where clear waters and a calm sea make ideal collecting conditions.

ward, away from the rest of the head, on long gray tubes. The rubbery, hoselike stalks extend so far that the comical manipulation of the eyes almost seems to be accomplished by remote control. But Tom Thumb's eyes are more for show than use. They are built in the form of the old-fashioned peep-show, and probably the molluscan brain can register only changes in light intensity and dim outlines of near-by moving objects.

The flats at Alofao are ideal living grounds for other sand- and mud-loving mollusks. The water is relatively quiet, but kept sufficiently fresh by changing tides. When the tide draws slowly back from a long, level sand bar, the sun readily warms the water. Microscopic organisms thrive, and seaweed growths become attached to every rock and nearly every shell. Here I found a splendid colony of nassa snails, *Nassarius globosus* Quoy and Gaimard. This half-inch, minutely corrugated gray shell spends its more active moments in sliding along the mud flats in hot pursuit of the falling tides, and again in returning with the rise. Nassas are not as sluggish in disposition as their muddy environment might indicate. They seem to be continually inching over the surface as fast as their one foot will carry them, and not one specimen could I find guilty of gathering moss. Their apparent rush through life is motivated by their appetite for dead flesh, and in their small way they help keep the beaches clean.

Eventually your eyes grow tired of the monotonous gray tone of the mud flats, and the tiny mollusks you seek fail to register in your brain. In front of Alofao Village you can desert the mud stretches and step a few yards to firm, sandy bottom. The white sand and clear lagoon are in startling contrast. The sunny brilliance of the crystal clear water can hold you with almost bewitching fascination, and even the most indifferent collector is impressed by the possibilities in the white sand carpet. Hours of soul-satisfying swimming often become an irresistible overindulgence in collecting, ended only by the warning of oversoaked, wrinkled hands.



Animal of the small conch, *Strombus canarium* Linné, showing the eye (*e*), foot (*f*), mouth (*m*), and operculum (*o*). (From *Voyage de La Bonite*.)

Bathing trunks, water goggles or a face mask, and a collecting bag tucked under the belt are the open sesame to a new kingdom of profitable hunt-

Above: A tiny bay set in a matrix of Samoan hills and Pacific seas. *Below*: A salty breeze and shallow reef waters invite the shell collector.

ing. William Beebe has long extolled the advantages of a diving helmet, and for purposes of strolling through the coral gardens under thirty feet of water, this type of diving is ideal. But in a shallow lagoon, in most places no deeper than four or five feet, helmet diving, of course, is impractical. Even in deeper waters, helmet diving is not suitable for collecting sea shells. Those helmet divers who have attempted to leap in sprightly fashion over a jagged coral head know from experience the futility of rapid motion. If they lean over to pick up an object, the water splashes up inside the helmet to blot out all vision. But for lagoon collecting goggles allow almost unlimited freedom

of action. You can make your way about in fish fashion, exploring any part of the bay in three-dimensional travel. It is true that each submergence is limited by the diver's lung capacity, but it is surprising in how few days of continuous diving one's underwater endurance can reach the two- or even three-minute mark. The experienced goggler spends most of his time cruising by means of breast strokes over the surface, remaining buoyed up by a large lungful of air. Using leisurely strokes, the swimmer may continue his cruising tactics for hours without tiring, lifting his head out of water occasionally to gulp in fresh air. The lungs may be used like the ballast tanks of a submarine. Emptying the lungs of air, the swimmer can drop to the bottom like a rock. A quarter lungful will permit cruising just over the sandy bottom. When an interesting shell is discovered, a complete emptying of the lungs will settle the observer down on his elbows on the sand.

Just as woodsmen make use of their knowledge of animal trails over the snow, marine collectors can reconstruct the drama of life on the lagoon floor. I remember cruising one morning along the surface in search of shells when I came across a neatly made track—a miniature and exact copy of a tractor trail. The tracks of sea shells in the sand are usually short, no more than six to ten feet in length, but this one led off into the hazy distance across the lagoon desert. As I followed along its course, I noticed another, similar trail converging toward the first one. Then still another appeared. All were apparently headed for a rendezvous at the other end of the lagoon. Finally I reached the mecca, and found the ruins of what had once been a large, long-spined sea urchin, *Diadema*. Little of the body was left, only a scattered patch of broken, purple, needlelike spines. Between the spiny rubble and a near-by section of the coral reef lay a wide, hazy trail, indicating that the urchin had wandered away from the protecting reef sometime during the night. A few broken spine tips at the base of the coral wall were evidence that the urchin had been dislodged by wave action or by some unknown creature, and had been injured in its fall to the lagoon floor. There were signs of a tussle near the ruins, where possibly a large fish had attacked the helpless urchin.

But the mystery of the origin of the tiny tractor trails was still not solved. I saw that the trail-makers had been enticed to join the carnage and, having had their fill, had moved off again. At last I tracked down the agents, discovering at the end of each trail a large hermit crab scurrying along over the sand with an old, weather-beaten shell on its back. By far the longest tracks are left by hermit crabs, which seem to be engaged continually in important missions at the opposite end of the lagoon. The great length of their trails is due primarily to the speed at which they scamper across the bottom. I say "speed" because they travel ten times as fast as the average gastropod, and to walk around the world without rest the crab would take only forty years, the snail perhaps four hundred.

I also saw near the urchin ruins a short wide trail not uncommonly found on the lagoon bottom. At the end of it, I found an adult moon shell, *Polinices mamilla* Linné, whose glossy white exterior is like the most perfect of well-finished chinaware. The foot and head of the animal, when its tiny internal ducts are filled with water, are quite easily twice the size of the shell. The creature is equipped with an additional propodium, or fore-foot, which it uses to shovel and push the sand up on top of its shell, Quite often the moon shell remains dormant for a day or two at a time, but a small mound of sand will always reveal its presence.

No mollusk trail is indelible on this sandy slate of the seashore, but the gastropod with the widest foot, or largest shell, will always make the most lasting impression. On a normally calm day, a well-impressed track will not be obliterated by drifting sand within twenty hours, but during squally weather, when the lagoon's face is clouded with suspended particles of sand, such a track will be covered in one or two hours.

One of the prettiest and daintiest shells of the South Seas is the olive-shaped mitre, *Imbricaria olivaeformis* Swainson. The shell is a little larger than a bean, and is brightly hued with an enamel coat of golden yellow. Its smooth rounded tip, or spire, is lacquered over with a splash of deep royal purple. I am tempted to dub this little mitre the "Who-done-it?" shell, for its short, yard-long trail is nearly always in the form of a question mark. The hook of the question mark is fine and distinct, as if made by the tip of a very sharp pencil. In the spot where the mitre shell is busily inching along, a tiny hump of sand sticks up to form the dot of the question mark. Many shells have a characteristically curved or meandering trail, but why "Who-done-it?" should start to make a circle and then straighten out its course to complete the form of a question mark is one of nature's riddles.

The greatest thrill I had while hunting mollusks in the lagoon area of the bay was in plucking huge

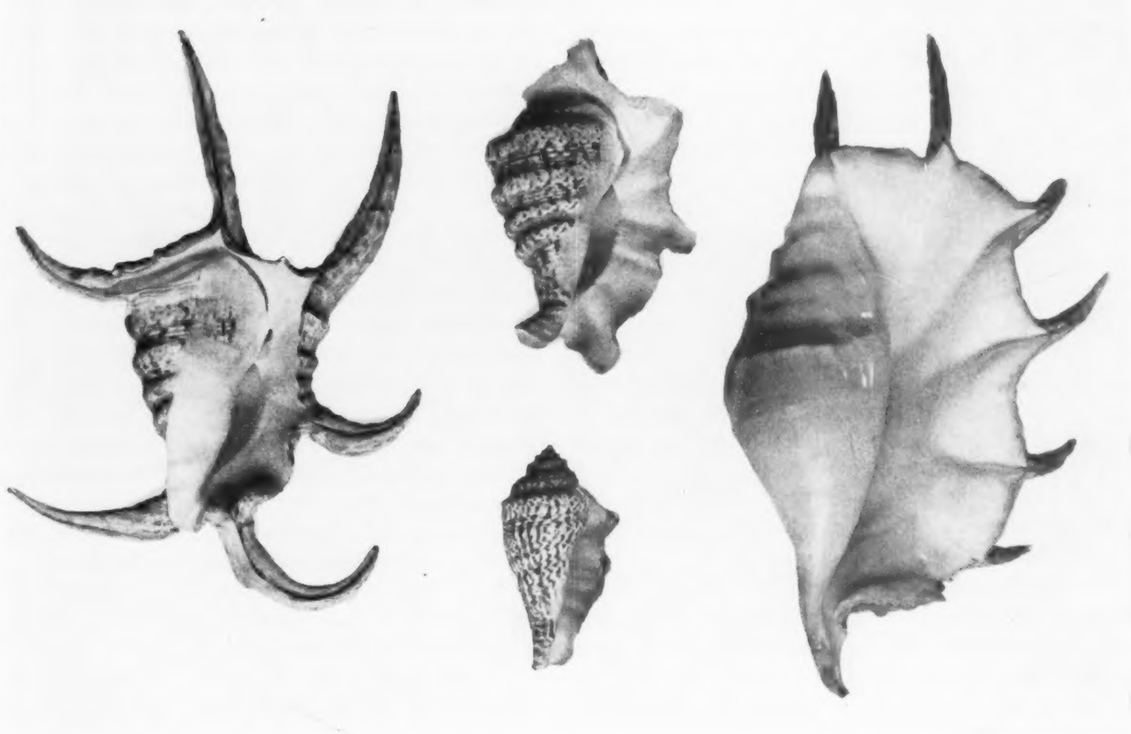
marlinspike shells, *Terebra maculata* Linné, out of the sand. It was much like pulling deep-rooted carrots out of the garden soil. The marlinspike is four to eight inches in length and shaped very much like a long, drawn-out cornucopia. The smooth, heavily enameled whorls are colored a creamy white, with a series of red-brown splotches spiraled around the entire shell. The animal digs down deep into the sand, so that only an inch or so of the spire projects above the surface.

After weeks of searching for mollusks in mud and sand, on rock shelves, under coral heads, on mangrove stumps, even in fish stomachs, I thought that every collecting possibility had been exhausted. Then one day, rummaging in the water near the mangroves, I picked up a white, short-spined sea urchin (*Tripneustes*), to inspect the delicate system of tubular feet on its underside. Nestled down between the tubular feet and very short spines was a pair of thin, nut-brown clams about the size of the nail on my little finger. I checked with the other sea urchins and confirmed my suspicion that the small fingernail clams were not accidentally adhering to the urchin but were in fact symbiotic clams found nowhere else in the bay nor on any other creature.

A billion years ago, perhaps, the survival rate for such tiny clams was greatest in the immediate vicinity of the white sea urchin where vegetable debris was richest. Teleologically speaking, we might say that some bright fellow, an evolutionary-minded bivalve, eventually decided not to wait for the creeping sea urchin to come, but instead clambered aboard the lumbering, seaweed grist mill. Today, a fingernail clam is in danger of starvation if once detached from its host. Such is the price of luxurious specialization.

The five-armed starfish, *Ophidiaster*, in the mud

Five species of Indo-Pacific spider conchs, *Lambis*. *Top, left to right*: *Lambis lambis* Linné, *L. arthritica* Röding, and *L. scorpio* Linné. *Bottom*, first pair: *L. millepeda* Linné; second pair, *L. crocata* Link.



The two largest species of spider conchs, *Lambis*. *Left*, and two young specimens, *center*: *Lambis chiragra* Linné; *far right*: Bryon's spider conch, *Lambis truncata* Humphrey.

regions, also turned out to be an unexpected source for a rare parasitic gastropod. On the bottom sides of the arms, wedged between the hundreds of small tubular suckers, were to be found some white shells, each about the size of a grain of rice. Specialization has gone to extremes in the case of this tiny *Eulima,* or "suck-shell." The head and mouth of the snail have been drawn out into a single slender tube for the purpose of tapping the rich body fluids of its host. Some species of "suck-shells," such as *Stylifer,* have taken on the strange habit of burying themselves deep in the flesh of the starfish as a precaution against being rubbed off. The great holothurian, or sea-cucumber, which resembles a black, oversized cucumber, has a much more uncomfortable time with its parasite. In this case, the "suck-shell" crawls right into the sea cucumber and jabs its proboscis into the intestinal wall. The highly nutritional waterworks are tapped for food, and enough sea water passes through the holothurian to give adequate aeration to the snail.

Mollusks do not always act as the villainous parasite in these strange associations. Sometimes the snails themselves act the gracious host, as in the commensal association between the large queen conch, *Strombus gigas* Linné, and the small conch fish of the West Indies. The first conch fish to take refuge within the folds of the conch shell was indeed a desperate or a very brave animal. When the conch is extended, there are no more than three or four cupfuls of sea water between the conch's foot and mantle. When the animal withdraws into its shell, this volume is reduced to about a single cupful. In time of trouble, the inch-long fish flits into the protective folds of its molluscan host. Passing sharks or barracuda rarely give the hulking shell a second glance, much less attempt to crack it open. Sealed in the dark chamber of flesh, the fish must wait until the conch sees fit to protrude its massive foot once more and liberate its guest.

A few days after my arrival in Samoa, I heard of Frank, a native renowned for his diving ability, who had been with Roy Waldo Miner on the *Zaca* expedition in the Polynesian Islands. When word reached Frank that I planned to go diving in the deep channels of Fagaitua, he immediately recessed from his construction job with the Navy at Pago Pago, and returned to his native village of

Alofao. He loved fishing and diving almost as much as he did his wife and children; and he understood the urge that impelled naturalists to spend their time collecting shells and fish.

On our first day of diving we chose the middle channel directly opposite Alofao Village, because it was wide, deep, and relatively quiet. Frank struck out with great powerful breast strokes until he reached a spot midway in the channel. On either side of us, a dozen yards away, were the two reef walls of the channel. It was low tide, and a great section of the reef was out of water. The current in the channel at the time was almost negligible, and the swell of the ocean was little more than a gentle heave that sighed against the dripping sides of the reef. The floor of the channel sloped very gently down from the sandy bottom of the lagoon to a depth of forty feet where we were now treading water, and thence on out to the hundred-foot depths of the outer bay.

From the distant cliff road the waters of the channel are a beautiful blend of greens, blues, and purples—delicate shades determined by the type of bottom, whether sandy or rocky, and by the depth of water. But here, bobbing on the surface with my begoggled head under water, the view was of a totally different kind of beauty, possessing something of the grandeur we find in a canyon when we stand on its rim and look far across to the other side. I felt as if I were looking down into a gigantic aquarium. Behind me, the sandy floor sloped upward into the opaque yellowish haze of the lagoon; in front, the subdued light of bluish water blended off into purple nothingness. Yet the water in the immediate vicinity was so clear I could see every detail of the bottom and of the coral sides.

The utter silence was as much a part of this strange kingdom as the softened light. All sound was blotted out beneath the surface—only the senses of sight and touch could be used in observing this new world. It was annoying at first to be so limited, for in a new and strange environment we humans like to sniff the air for odors and cock our ears for sounds. We wish to associate new experiences with familiar sounds and smells.

To Frank, all this was as familiar and commonplace as subway sights to a New York commuter. Before my untrained eye could pick out the main object of our search—a mollusk worth collecting—Frank had already started his first dive. I felt rather puny in physique and ineffectual as a swimmer as I duck-dove at the surface and attempted to follow. Within half a minute my breath was beginning to shorten and my strokes to weaken. I saw now that Frank was headed for a large shell of some sort, but my interest in reaching the bottom turned into a panic-stricken urge to get back to the surface for air. I turned around and shot for the surface, emptying my lungs on the way, and the instant I splashed through the channel ceiling, I was thankfully gulping fresh air.

In a few moments, Frank also surfaced with a splashing explosion, and triumphantly waved above his head a large specimen of the spider conch, *Lambis lambis* Linné. This was the first species of its genus, with live animal, that I had seen so far on my Pacific sojourn, although it is a fairly common shell of the conch family, both in museum collections and in its natural reef haunts. Personal introduction to an old friend of museum cabinets is a thrilling experience among all museum workers who go into the field. The most staid of fish picklers will register delight upon seeing his first dolphin or sea horse in action.

Private shell collectors strive to obtain at least one example of these handsome spider conchs for their collections, for the shell is large, brightly colored about the mouth and grotesquely ornate, with long, curved spines that resemble spider legs. There are about half a dozen species of *Lambis* in the tropical areas of the Pacific, and each kind possesses a characteristic type of spine and distinctive color pattern. One species may bear a set of six gracefully curved and slender spines, another may carry eight thick, contorted spines. The adults of Bryon's spider conch, *Lambis truncata* Humphrey, are massive and nearly a foot in length, with the under surfaces glazed with a white enamel, whereas the orange spider conch, *Lambis crocata* Link, rarely exceeds four or five inches in size and is brilliantly painted underneath with a cream orange. Yet all spider conchs in their young stages look very much alike and, indeed, can scarcely be distinguished from the young of their cousins, the *Strombus* conchs. It is not until they approach maturity that the shells of *Lambis* begin to lose their normal conchlike appearance, and continue to develop the lip of the shell into long, drawn-out fingers, or spines. It is a matter of conjecture whether these shelly appendages are of importance as a form of camouflage or as a protection against predatory fish. For that matter, they may have no survival value at all.

Frank and I continued our exploration of the

channel. By confining my diving activities to depths of less than twenty feet, I was able to reach the few shells that could be seen from the surface. Additional small but choice mollusks came to light by turning over dead slabs of coral rock on the bottom. It was on one of these longer dives that I chanced to spot, off to one side of the channel in a small coral alleyway, a huge helmet shell, *Cassis tuberosa* Linné. Apparently, the helmet shell had wedged itself into this tight corner on a recent foray for sea urchins, and in its slow molluscan way had not yet explored the possibilities of backing out. On the next dive, I swam directly over the trapped mollusk, using both hands to wriggle it loose.

An armful of *Cassis* such as this necessitated a trip back to the beach, where it was added to our growing pile of spoils. Frank had already come ashore several times, and I now saw him returning for the fifth time with a number of large shells tucked under the cloth around his waist. Two hours of diving in deep water is rather strenuous, and we were both glad that the incoming tide and the accompanying currents in the channel were bringing our operations to an end for the day.

One of my favorite Samoan families lived in the first large hut just beyond the beach, and invariably I headed for the cool shade of their *falé,* where I could sit at rest on the grass mats with my back to a wooden pillar. While I munched cold fish and hot, freshly baked breadfruit, Samana, the head of the family, would look over my catch and make solemn comments on the rare or common occurrence of each species. "Ah, yes, this is a *papatuka,*" he would remark, holding up the spotted cone shell, *Conus pulicarius* Hwass. "I know a place where there are as many as the leaves on a coconut tree. See, over there." By then, my strength would be back and my hunger satisfied, and, as I followed Samana's pointing finger in the direction of the open sea, I would begin to plan tomorrow's collecting trip.

Samoan scenes reproduced by courtesy of Leonard P. Schultz, Smithsonian Institution.

INSTRUMENTATION AND CYBERNETICS

JOHN D. TRIMMER

Dr. Trimmer (Ph.D., Michigan, 1936) joined the Department of Physics at the University of Tennessee in 1946 after six years at the Massachusetts Institute of Technology and three years at Oak Ridge. His article is based on an address given at the NACA Langley Memorial Laboratory on July 5, 1949.

IT IS necessary to define, not only the new domain of cybernetics, but also the older domain of instrumentation. It remains to be seen whether this need can be fully met. One might expect that the newer term, having been coined *ad hoc*, would be more clear-cut than the older one, which "just grew." This expectation is hardly borne out. In fact, in probing after an understanding of these terms and the relations between them, it becomes evident that the basic concepts needed to establish satisfactory definitions of either one have not yet been too well clarified.

The following discussion, therefore, may not always be in closed form; but it will represent an honest effort to bring out into the open some of the questions that seem to need answering. The first of these concerns the nature of instrumentation.

WHAT IS INSTRUMENTATION?

The history of twentieth-century technology shows that one of its most rapid developments has been that of measuring instruments and automatic controls. This development followed a certain natural course of evolution, beginning with emphasis on indicating instruments, which left on the human observer both the burden of remembering the information provided by the instruments and the burden of acting upon that information. Next came emphasis on recording instruments, which might be described as instruments with built-in memory. The third and final stage of this sequence gives emphasis to the automatic control, in which the instrument takes some action, presumably more or less similar to that which the human observer would take if he were acting upon the information furnished by the instrument—that is, action toward an end desired by the user of the machine. According to present terminology, the device is called a *regulator* if the action is directed toward a goal that remains fixed for relatively long times, and a *servomechanism* if the goal changes somewhat rapidly.

The early stages of the growth of instrumentation were related to the various branches of engineering: instruments such as the engine indicator came to be a subject of study in mechanical engineering; the various electrical instruments were incorporated into electrical engineering; somewhat later, the problems of aircraft instrumentation found their place in aeronautical engineering. This early diversification of indicating instruments was gradually modified by two important influences. One was the increasing emphasis on automatic control; the other was a synthesis of the features common to various kinds of instruments—electrical, mechanical, etc.—into a single unified body of ideas. Although these influences, particularly the second one, have not yet had their full effect, there is today widespread agreement that instrumentation is considered to deal in general and in detail with all kinds of instruments—indicators, regulators, and servos.

Such a definition of instrumentation appears to be quite satisfactory in giving to the field of instrumentation internal coherence—that is, in stating what instrumentation should include. There remains, however, the question of external coherence—that is, to what does instrumentation belong? Of what is it a part? This question has not been answered. It deserves careful consideration.

One approach has been worked out by the author during the past several years in teaching a course on instrumentation in the Department of Physics at the University of Tennessee, and it is expounded in a forthcoming textbook written for this course ("Response of Physical Systems," to be published by John Wiley & Sons). A brief statement of this point of view will be given here. It is based on a certain pattern of experience (Fig. 1). A system, of nature unspecified, is subject to an input, or "forcing," and gives an output, or "response." A general study may be built up around this pattern, including in its domain not only physical systems, such as instruments, regulators, and servos, but also biological and sociological entities. This general study will here be called "system response." (Basic principles of system response are outlined in the first chapter of "Response of Physical Systems.")

Thus, one answer to the problem posed above is to say that instrumentation is a part of the subject of system response, for the subject matter of instrumentation fits neatly into the pattern of Figure 1. Instruments, regulators, and servos are systems having forcing and response. For instruments, the forcing is the true value of the quantity being measured and the response is the reading obtained. For regulators and servos, the forcing is the desired value of the quantity being controlled and the response is the actual value.

Any pleasant feeling of accomplishment at having arrived thus far is short-lived, however, for one is immediately led to ask: Of what is system response a part? And it is at this point that it is convenient to refer to the beginning of this paper and to ask the second principal question, concerning the nature of cybernetics.

WHAT IS CYBERNETICS?

The English word "cybernetics" was formed from the Greek word (*kybernetes*) for "steersman," and was presented to the public in a recent book by the mathematician Norbert Wiener (*Cybernetics.* New York: Wiley, 1948). The subtitle of this book, *Control and Communication in the Animal and the Machine,* suggests a simple way of displaying the domain of cybernetics (Fig. 2). The chief difficulty in attempting this display is in finding single words, or even single terms, to describe each of the four quadrants of the diagram, particularly the lower two.

Taken in a somewhat modified and generalized sense (indicated in the figure by the quotation marks), the words "thought" and "action" may be considered to refer to the totality of problems arising, respectively, in animal communication and in animal control. The word "instrumentation" in the sense defined above seems to represent admirably the machine-control quadrant. Although the term "communication engineering" appears to be a suitable synonym for machine communication, it is worth noting that Professor Wiener puts considerable emphasis on computing machines, or, more generally, "thinking" machines. One is therefore faced with the alternatives of including such machines under communication engineering or instrumentation, or else of replacing "communication" by some more general word such as "thought." One feels here a certain awkwardness which suggests that the contents of cybernetics might profitably be grouped and arranged somewhat differently. A suggested way of doing this is presented at the close of this paper.

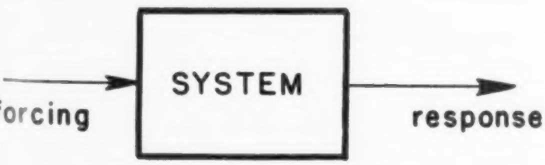


Fig. 1. A pattern of experience.

	COMMUNICATION	CONTROL
MACHINE	communication engineering	instrumentation
ANIMAL	"thought"	"action"

Fig. 2. The domain of cybernetics.

Both the derivation of the word "cybernetics" and the contents of Wiener's book put much emphasis on feedback. This corresponds closely to what has happened in the narrower field of instrumentation. In the twentieth-century evolution of instrumentation discussed above, an outstanding feature has been the increasing prominence of feedback. This has gone so far that many people accept feedback as the defining aspect of regulators and servos. With this viewpoint the author cannot agree. However convenient and widespread the use of feedback may be in the practical execution of actions directed toward desired ends, to make it the *sine qua non* of such devices seems to be a logical anomaly, comparable to defining mammals as "animals with four legs." Similarly, in the broader domain of cybernetics, the role of feedback appears to be more incidental than the initial interpretation has implied.

Referring to the question of what is instrumentation a part, it is clear that two answers have been found. On the one hand, instrumentation fits into the subject matter of system response; and, on the other hand, it is clearly a part of cybernetics. This situation must mean either that one of the two subjects, system response and cybernetics, is larger than the other and so includes the other, or else that both may be regarded as more or less distinct parts of a still larger discipline, not yet specified. Before attempting to decide between these alternatives, it may be well to look more closely at the way instrumentation actually fits into the two larger fields of system response and of cybernetics. This may be done by considering a particular problem in instrumentation.

THE PROBLEM OF INSTRUMENT EFFICIENCY

When a measuring instrument is to be designed, or is to be selected for purchase or for use, the question arises as to just how many distinct speci-

fications are involved. For present purposes, one can draw a somewhat arbitrary line to rule out economic and aesthetic factors, such as cost, convenience, appearance, ease of maintenance, etc. Then for instruments of a given class—that is, for instruments that measure any one given physical quantity—there appear to be three principal specifications. One of these is accuracy, and a second one is range. On these two there is doubtless wide general agreement. But the third specification is less familiar and, perhaps, more controversial. It may be called "instrument efficiency," or, in context where it will not be confused with the familiar thermodynamic term, simply "efficiency."

Instrument efficiency may be described as a measure of the magnitude of reading obtained per unit of energy exchanged between the instrument and the system being measured. Thus an instrument would have infinite efficiency if it could furnish a reading without having any interaction (energy exchange) whatever with the system on which the measurement is made. It is a complicated problem to find the best way of putting this idea of instrument efficiency into terms of an exact definition. (This problem is discussed elsewhere and will not be treated here.)

More pertinent to present purposes will be an effort to regard the concept of instrument efficiency in the general light of system response, and in the general light of cybernetics. From the point of view of system response, an instrument, as noted above, may be regarded as a system subject to the forcing of the true value of a physical quantity and giving as response a reading of the value of that quantity. Good accuracy requires that these two values agree as closely as possible, and wide range requires that the agreement hold over a large interval of values. These two specifications, therefore, seem rather simply related to the pattern of system response. But instrument efficiency does not seem so simply related, since it calls into the picture the hitherto extraneous element of energy or power. (The choice between energy and power in the exact definition is one of the complications mentioned above.)

A little reflection shows, however, that all three specifications are extraneous—none of them is a necessary part of the pattern of Figure 1. These desiderata together constitute something imposed on the general pattern to make it fit the case of measuring instruments; they constitute the design criterion for instruments. The general agreement—that accuracy, range, and efficiency are desirable attributes of the relation between forcing and response—is indeed a most necessary condition for advance in the art of instrumentation. By contrast, in the design of economic, political, or sociological systems, to all the other difficulties of design and execution there is added the overwhelming primary difficulty of obtaining general agreement on what constitutes desired performance of such systems. Yet all detailed design discussion of any system is surely somewhat inane unless it can be based on a clear expression of the performance expected of the system.

For any system, physical or otherwise, one can easily envisage concepts of "accuracy" and "range." Accuracy is simply a measure of how closely the actual response of the system conforms to the desired, or standard, response. Similarly, range can be defined in almost the same words as for instruments. But what is in general analogous to instrument efficiency? One comes here again to a suggestion of strangeness, of not being quite satisfied with this concept of instrument efficiency.

Suppose now that the concept is regarded in the general light of cybernetics. Communication, which according to Figure 2 is half the domain of cybernetics, may be described as the transfer of information; so that the idea of information is a very fundamental one, cybernetically speaking. The general problem of defining "amount of information" is discussed at some length in Wiener's book. Roughly speaking, the amount of information in any given item is taken to be proportional to the number of decisions (between alternatives) required to specify the item. Applying such a definition of information to the process of measurement, one may think of the output of an instrument as so much information about the quantity being measured. Then it is natural to define instrument efficiency as amount of information yielded by the measurement per unit energy interchange with the measured system.

But the amount of information is related to the number of significant figures obtained and so to the accuracy of the measurement; hence the concept of efficiency thus defined would not be independent of the concept of accuracy. Moreover, this definition of efficiency does not give any weight to the magnitude of the reading. Thus a d.-c. voltmeter reading of 100 volts to two significant figures would represent the same amount of information as the reading of 1 volt to two significant figures. Let I denote this amount of information. Suppose the 100-volt reading is taken at full scale on a meter having resistance of 200,000 ohms and the 1-volt reading at full scale on a meter of 20 ohms resistance. Then the efficiency for the two meters is the same, since I is the same and the input power is the same, namely, 50

milliwatts. This situation violates the intuitive feeling one has that a 2,000 ohm-per-volt instrument (the 100-volt meter) should be credited with higher efficiency, according to any definition, than one with 20 ohms per volt (the 1-volt meter). This "ohm-per-volt" specification is identical, for the special case of voltmeters, with the first general definition given above—reading per unit energy—evaluated at full-scale reading.

In summary of this brief discussion of instrument efficiency, it must be admitted that no fully satisfactory definition has been presented. The definition as the ratio of reading to energy exchanged seems to be a useful one, but the problem needs further study. In any case, it is hoped that this discussion of the problem has served to illustrate some of the relations among instrumentation, system response, and cybernetics.

THE ULTIMATE SUPERINSTRUMENTATION

At the outset of this article it was implied that more questions would be raised than would be answered. A fundamental question, which was brought up but not answered, is the one concerning the relation between system response and cybernetics. Perhaps some sort of answer can be found. What has happened is clear enough—instrumentation has grown, and is still growing, into some kind of superinstrumentation. Or, expressed another way, instrumentation is the core around which a much larger structure of ideas is crystallizing and taking shape. In endeavoring to promote this crystallization, the author, interested primarily in measuring instruments, arrived at the mode of thinking described above as system response; Wiener and co-workers, preoccupied primarily with communication, servomechanisms, and computing machinery, arrived at the concepts of cybernetics. Are these simply two ways of regarding one and the same thing? Or are they two different cross sections of a larger entity, which transcends and encompasses them both? It may be that this question must be left to the future for final answering. But further speculation on the matter will not be amiss here.

Can system response be said to include all of cybernetics? Communication systems and feedback systems fit well enough into the pattern of system response, but computing machines and "thinking" machines do not fit so readily. There seems to be need here for distinguishing devices which *transfer* or *transform* information from devices which *generate* information. Efficiency of the former would be expressed in terms of how little information they *lose*; that of the latter by how much information they *gain*. Since a measuring instrument may be thought of as generating information, the problem of fitting computing machines into the pattern of system response may be closely related to that of fitting instruments into the pattern; and the problems of instrument efficiency and of computing machine efficiency may have much in common.

If there is some doubt as to whether system response can include all of cybernetics, there is also some doubt about the converse. Even when cybernetics is taken in the broadest sense of covering communication and control in the animal and the machine, with no restriction traceable to anyone's emphasis or interpretation thus far given, it would seem that the pattern of system response can be readily applied to many situations which are quite outside the domain of cybernetics. Any number of physical, chemical, and meteorological phenomena could be mentioned as examples—in fact, any situation where actions run their course, without the communication of information or the control of the actions toward a desired end. But there is one tour de force capable of reversing this situation. That is to note that all *knowledge* of the pattern of system response is achieved by the generation of information by measurement or observation; so that if the generation of information is made part of cybernetics, then system response might well be considered a part of, or a method in, cybernetics.

This suggests that the ultimate superinstrumentation taking shape might be a broadened cybernetics, as follows:

1) *Observation*—generation of information by:
   - instruments
   - sense organs
   - computing machines
   - brains

2) *Communication*—transfer of information by:
   - devices of communication engineering
   - nerves
   - language

3) *Control*—transformation and use of information by:
   - regulators and servos
   - organisms
   - societies

Such a threefold cybernetics—the science of observation, communication, and control in the animal and the machine—would seem to be a unified, coherent discipline, capable of standing on its own feet and taking its own place, a science among the sciences.

SCIENCE ON THE MARCH

GRASSLAND RESEARCH IN BRITAIN

Britain's agricultural program aims at raising food production by 20 percent within five years, to enable her to feed four million more people from her own resources. It is planned to increase home production of cereals, in order to reduce the demand for imported grain, and to step up considerably the output of meat and dairy products.

To achieve the increase in meat and dairy produce, more animal feeds must be found. To avoid a heavy increase of imported feeds, an intensive program of home grassland development is being aimed at, with a considerable increase of silage making and grass drying. The climate and soils of Britain combine to make grass the major crop and one which must be exploited to meet the changing needs of agricultural production.

Britain's experts, reporting last April on the best means of applying research to the problems of increasing productivity, advised that the achievement of the agricultural program was well within the bounds of technical possibility. Under good management the nutrient value of an acre of grass could be increased two or three times, and the foodstuff value of the product could be equated with that of the best coarse grains. By concentrating on a program of intensive development of grassland, it should be possible to obtain within the next four years a 20 percent increase in the total yield.

The achievement of this program depends to a great extent on the work being carried out at the Research Establishments of Britain's Ministry of Agriculture. Among these is the Grassland Research Station at Stratford-on-Avon, Warwickshire, which is concerned with the problems of seed mixtures and leys in relation to soil fertility, sward management, and crop yields; the grass-legume ley is the main crop under study.

Recent researches have shown that the energy value of the grass-legume ley, when farmed at high level, is greater than almost any other crop that can be grown on United Kingdom farms. In terms of total dry matter, a productive ley will produce upward of three tons per acre of nutritious herbage in a normal season. Such herbage may have a starch equivalent of 65–70 percent, which means that the total starch equivalent per acre will be of the order of two tons. This compares favorably with a well-farmed crop of potatoes yielding, say, ten tons per acre, or with a 30-cwt. grain yield of wheat or oats. These comparisons are shown in Table 1, where it will be seen that none of the cereals and few of the root crops bear comparison with the starch equivalent and dry matter yields of good grassland.

The figures shown in Table 1 indicate:

a) High yield of starch and protein equivalents in grass compared with other crops.

b) The figures for oats, beans, kale, and potatoes are reasonably high averages for the better class of farm. Even kale is not as high in protein equivalent as is grass, although it may be higher in starch equivalent.

In making the comparison, the starch equivalent of the grass has been taken at 60 percent, which is low for a normally well-managed ley. The oat crop has been taken at 24 cwt. of grain for 36 cwt. of straw, but even if these figures were based on a 30-cwt. crop of grain and a 2-ton crop of straw it would still mean that oats are far lower in starch equivalent per acre than even a moderately good ley.

When we turn to the figures for protein equivalent the differences in favor of the grass-legume ley are found to be even wider. None of the cereals, pulses, or root crops approaches in protein equivalent per acre that produced by a ley in full production. Kale is the only crop which gives a protein equivalent production figure of the same order of values as the grass crop. These facts are of first importance, for they show that well-managed grassland can be made to produce more animal feeds than the normal run of arable crops used on mixed and livestock farms.

We can now turn to the question of the deficiencies and the *contra* aspects of the grass crop. The major point *contra* lies in the fact that grassland is extremely seasonal in its productivity. There is a long dormant period in winter from October to March or April, followed by a spring flush during May and June, when aggregate production from grasslands is considerably in excess of current requirements. The problem of the spring flush, therefore, is one of efficient conservation rather than of production. Traditionally, this

TABLE 1

YIELD PER ACRE OF DRY MATTER, STARCH EQUIVALENT, AND PROTEIN EQUIVALENT FROM LEYS AND OTHER FODDER CROPS IN BRITAIN

	Yield Per Acre		
	Dry Matter	Starch Equivalent	Protein Equivalent
	cwt.	cwt.	cwt.
Perennial ryegrass (S.23) and white-clover (S.100) ley (G.R.S. E.42, 1946)	74	44.4	8.9
Perennial ryegrass (S.24) and white-clover (N.Z.) ley (G.R.S. E.73, 1947)		29.0*	5.0*
Average good leys in Britain	60	36.0	7.2
Oats: 8 qr. crop = 24 cwt. grain			
grain	24	19.4	2.1
straw	36		
Beans: ton crop = 20 cwt. grain			
grain	20	18.9	4.4
straw	30		
Kale	25 (tons)	43.9	6.5
Potatoes	10 (")	36.0	1.2

*In 20 weeks starting May 20, 1947—a dry year—6 cwt. starch equivalent and 1.0 cwt. protein equivalent per acre would have been produced before May 20.

presummer peak production has been conserved as hay designed for winter fodder. One of the major problems is to provide for better quality in the annual hay harvest. On a small scale hay is made on tripods and with good effect on hay quality. In many districts, as in the north of England, hay is made into pikes on the field—later to be brought into the stackyard. Most of the methods of haymaking are expensive of labor, and the research problem is to find efficient means of mechanization in harvesting weather that is traditionally wet and uncertain.

In recent years, following the researches of S. J. Watson and others, there has been renewed interest in the production of grass silage. Silage makes the farmer less dependent on harvest weather, has cheapened considerably the cost of the grass harvest, and has also produced grassy material of higher protein content. An obvious point in favor of silage is that losses due to maturation of the crop are lessened as compared with hay. Serious losses have frequently occurred in the harvesting and storage of silage, but with improved techniques these losses can, in large part, be minimized.

More recently, the drying of grass by artificial means has gained prominence in Britain. The process is still largely experimental but shows considerable promise. The dried grass problem may be divided into two closely allied subproblems: (*a*) the production of grass for the specific use of the drier, and aimed at keeping the drying plant fully occupied with drying high-quality material over a prolonged period; and (*b*) the engineering problem of reducing not only the capital outlay on the plant, but also the costs of drying as such. There is little doubt that the artificial drying of grass has come to stay, particularly since protein-rich animal feeds are likely to be in short supply over the world as a whole for a long time to come.

In her grass crop Britain has potentially an abundant source of protein which, if properly handled and effectively conserved, could easily make her self-sufficient insofar as the requirements of a greatly extended livestock industry are concerned. In this connection the grass drier will play an important part, because losses due to both maturation of the crop and to harvesting can be deleted entirely. Britain has, then, the potentiality not only to produce good grassland but to conserve any excess over current seasonal requirements when the product is at a high level of quality as a protein-rich concentrate.

The proper conservation of the spring flush of grass production can help immensely to solve those problems inherent in the extreme seasonal distribution of grass growth. There are, however, other approaches to the problem which are the subject of much detailed investigation. For example, the low production period after midsummer on many of Britain's pastures can be dealt with by the use of specific seed mixtures coupled with appropriate grassland management and

manuring. In the drier parts of England, the normal ryegrass ley will usually enter a period of almost complete dormancy from early July until the end of August. On the other hand, leys based on lucerne with cocksfoot can be brought to a peak of production during this period. Another seed mixture that will produce an abundance of herbage at this time (July and August), if appropriately managed and manured, is that based on cocksfoot, meadow fescue, and timothy with white clover.

The winter (October to March) period of low production on our grasslands presents a much more difficult problem. Under most conditions in the United Kingdom, the well-managed ley will remain green throughout a normal winter but will be largely unproductive during that period. If especially managed, however, growth can be maintained fairly easily through October into November. The winter-green strains of cocksfoot, timothy, rye grass, meadow fescue, and some other species have assisted in an appreciable measure in lengthening the grazing season into the autumn months. These same seed mixtures are capable of producing also early growth in the spring, so that the winter period is fairly easily shortened; but there remains from late November to the end of March a serious gap which has to be filled.

During this period, quite obviously, the material conserved as hay, silage, or dried grass at the spring flush can be (and generally is) fed to livestock. Current researches, however, suggest that the present gap in winter grass (harvested either *in situ* by the animal or dried artificially) can be still further reduced. Here full use of the knowledge of the behavior of the winter-green grasses available must be made. An eye must also be kept open for any new material that may appear, whether exotic or as a result of indigenous plant breeding. Up to date, the greatest promise is shown by leafy strains in timothy, meadow fescue, and cocksfoot. The pasture strains of timothy (Aberystwyth S.48 and S.50), meadow fescue (S.215), and cocksfoot (S.143) show considerable promise in this respect and will produce green herbage in the period December to February. When these grasses are cultivated in wide drills (two-foot spacing) and effectively manured during the early autumn (August and September), they produce a wealth of leafage which remains tolerably winter-green, of high nutritive value, and very palatable, well into the early months of the year. Other grasses that show promise in this same connection are tall fescue (*Festuca arundinacea*), foxtail (*Alopecurus pratensis*), tall oat (*Arrhenatherum avenaceum*), *Phalaris tuberosa*, *Poa* species, and *Festuca rubra*.

To sum up, grassland research and its application to practice in Britain will tend on the one hand to close the gaps in seasonal production from grasslands so far as this is practicable. At the same time it is also concerned with the need to conserve material at the peaks of production during the summer and to harvest high-quality material as dried grass and silage, while at the same time preserving hay of medium to high quality. Grasslands can therefore be made to provide the bulk of rations for livestock, and at the same time full use can be made of the arable residues, both roughages and grain.

Clearly, arable and grassland farming are closely complementary in relation to the livestock industry; but grass will always remain Britain's major crop, not only because of the large proportions of land occupied by grass swards but also because well-farmed grass is the most productive crop in terms of energy values—starch and protein equivalents.

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PETROLEUM AND THE GROWTH OF THE PACIFIC COAST*

UNDER the American system of free enterprise, population and economic activity are able to expand in areas that are rich in natural resources. The Far West has been abundantly blessed with such resources, of which the principal ones are fertile soil, timber, water in considerable quantities, gold and other metals, and petroleum and natural gas. In addition, a favorable climate is a major asset and has been responsible for attracting people and industries in ever-growing numbers to the region. With energy and foresight, the Far West has made intelligent use of its many natural resources. The early impetus came primarily from the mining of gold and cultivating the soil, and the present century has seen the expansion or continuation of these activities, together with the intensive development of our other wealth.

* Based on an address presented at the Annual Meeting of the American Institute of Mining and Metallurgical Engineers, San Francisco, February 13–17, 1949.

It is hardly necessary to quote extensive statistics on the growth of the area, for the story is well known. A few population figures—covering the five states of California, Oregon, Washington, Nevada, and Arizona—will suffice. The present population is 15,000,000. Just before the war, it was less than 11,000,000; five years hence it is expected to be about 17,000,000. Our population is already more than double that of Sweden, greater than all of Canada, and ultimately should equal the present population of Mexico (Fig. 1).

The total value of the soil and subsurface products in the five states in 1947 was between four and five billion dollars. Superimposed upon these natural-resource industries are the products of our diversified manufacturing activities, which have a total value of about six billion dollars. When an area has great natural resources and fast-growing population and industry, its people are obviously in an economic position to purchase larger than normal amounts of many products. Such is the case here, as demonstrated by the fact that the average per capita income in these five states is about 20 percent higher than for the remainder of the country.

The activities represented by 15,000,000 people and the industries in which they are engaged require mechanical energy in vast quantities. It is interesting to compare the "source-of-energy pattern" in the Far West with that for the rest of the United States, and then to see how our large supplies of petroleum fit into that pattern. If the various fuels are converted into terms of British thermal units, it will be found that approximately 50 percent of the total energy delivered to consumers in the United States, exclusive of the five Western states, is supplied by coal. Petroleum supplies 35 percent, natural gas about 14 percent, and hydroelectric power only 1 percent. In striking contrast, the Far West is essentially an economy without coal, with only 5 percent of its total requirements being supplied by that commodity. About 4 percent is obtained from hydroelectric power, 22 percent comes from natural gas, and almost 70 percent from petroleum (Fig. 2). Thus the proportion represented by petroleum is just double that in the rest of the country.

It is readily apparent that, without large indigenous supplies of oil and natural gas, the growth of the Far Western states would have been much less dynamic than its history shows. With our other natural resources, a rapidly expanding population and industry, and with our lack of coal, the increase in demand for oil has been exceptional. Not only has it been necessary for Western petroleum to meet the fuel and lubrication needs of our

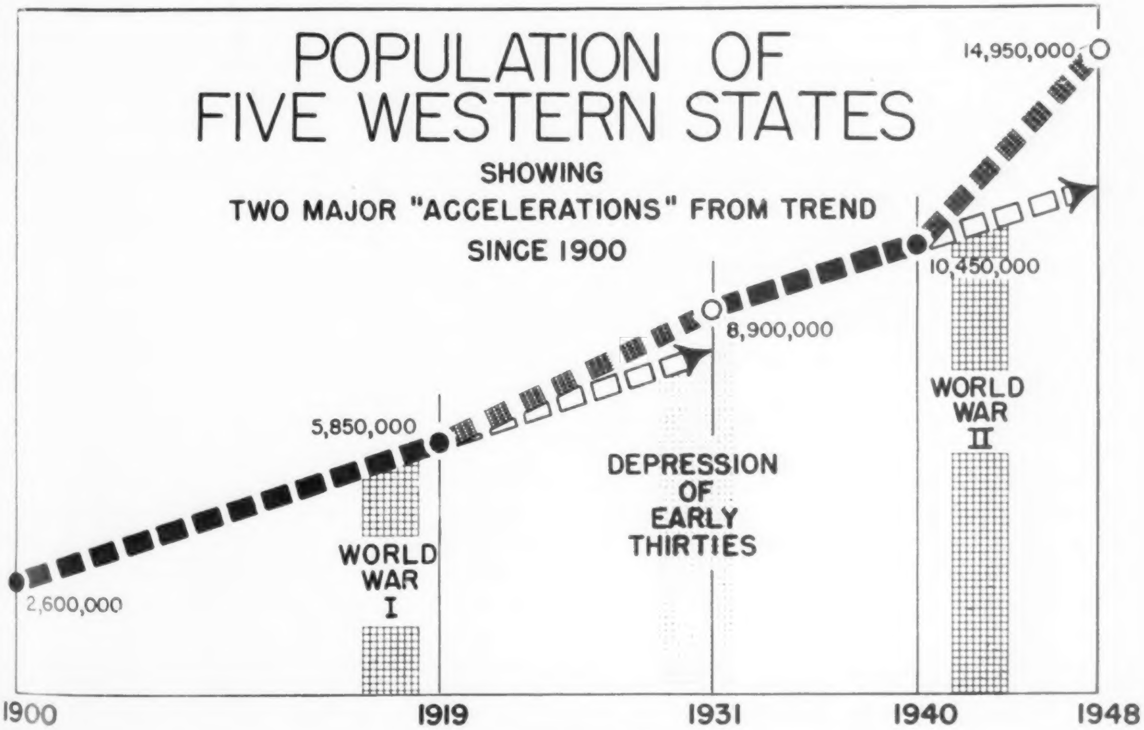

FIGURE 1.

millions of automobiles, trucks, buses, tractors, and other internal-combustion equipment, but, along with natural gas, it has assumed the burden of supplying our extractive and manufacturing industries, furnishing fuel for heating and cooking in our homes, and providing the fuel for the network of railroads that cover the broad expanses of the Far West. Recently, the electric utilities have made a heavy draft upon oil to operate their expanding steam-generating facilities. The large coastal and offshore shipping business is an important source of demand for fuel and Diesel oils; fuel oil bunkers the tankers that carry petroleum around the perimeter of the Pacific Ocean; the military requires approximately 100,000 barrels each day of various oil products; about 80,000 barrels of petroleum are required daily to meet our export demands; and Alaska and Hawaii draw upon this area for their liquid fuel requirements. Over all, a million barrels per day of raw materials must be produced, transported, refined, and distributed. In 1941, this figure was only 700,000 barrels.

These demands are expected to increase further in the future, although in less substantial proportions than in the past. One of the factors contributing to this situation is the rapid dieselization of the railroads, a trend that is becoming nationwide. Outside the Western part of the country, locomotives use principally coal; in the West, however, little coal is available for rail use, and, since one barrel of Diesel oil will do approximately the same work as four barrels of heavy fuel oil, the replacement of steam locomotives by Diesels results in very large declines in the consumption of heavy fuels in our area. In 1946, in the five Western states alone, the railroads daily consumed 112,000 barrels of heavy fuel. In 1948, they consumed just over 90,000 barrels, a decline of almost 20 percent in two years, despite high levels of railroad activity. Dieselization is not complete and, as it progresses, should cause further material reductions in rail use of heavy fuel oil, with relatively much smaller increases in the use of Diesel oil.

California in the past has produced ample supplies of crude oil and natural gas to meet the full requirements of our Far Western economy. In recent years, fears have been expressed that the supply may become inadequate to meet anticipated demands. Despite the fact that few large new fields have been discovered in the past decade, California increased its crude oil production from 631,000 barrels a day in 1941 to 913,000 in 1947. Concurrently, estimated proved reserves in the ground

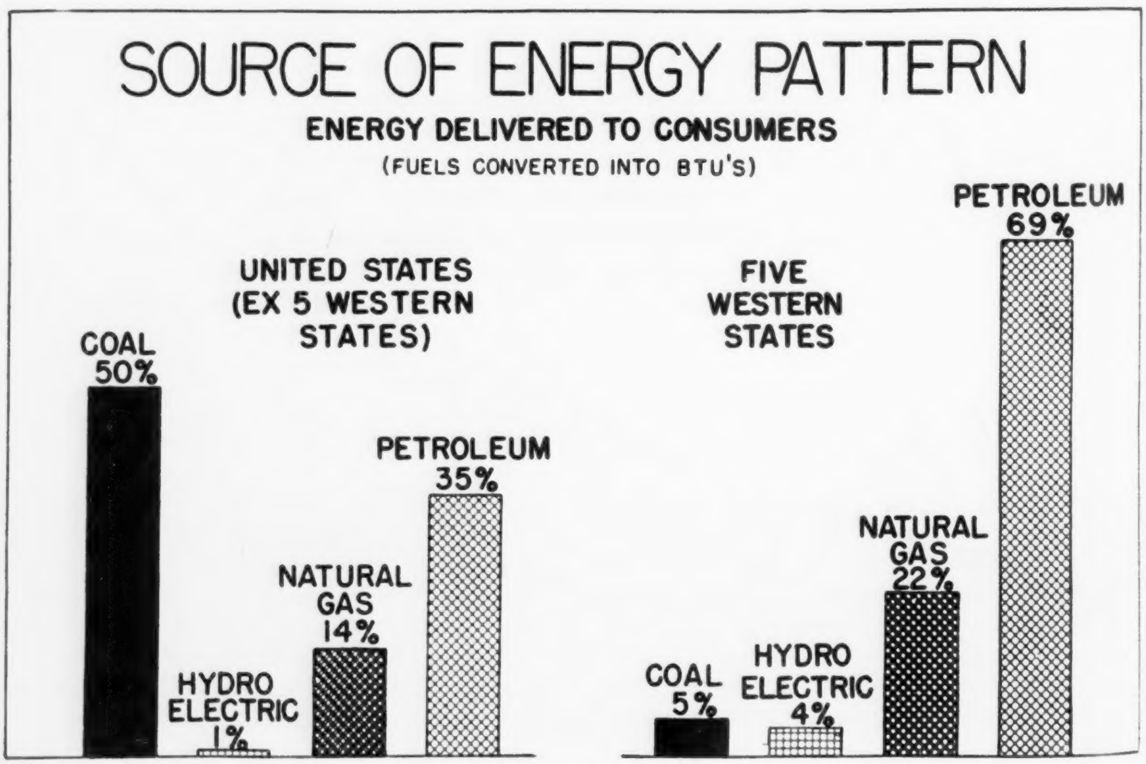

FIGURE 2.

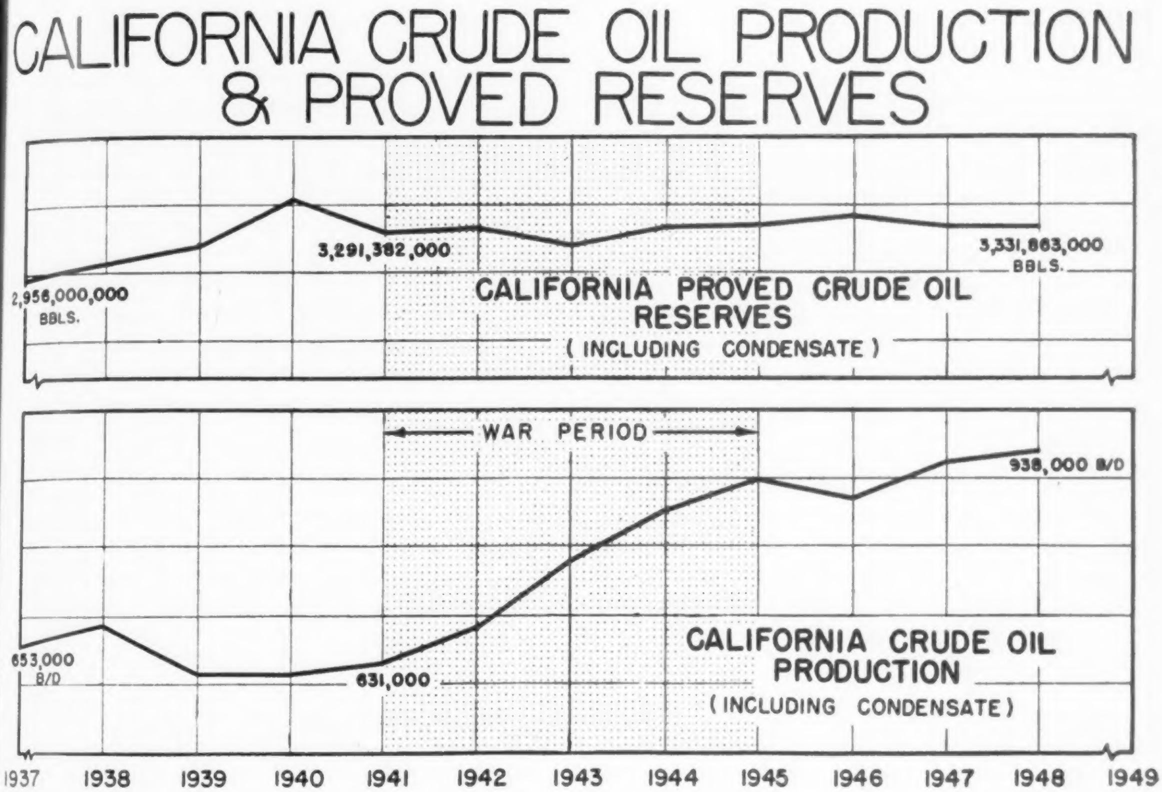

FIGURE 3.

remained substantially unchanged. Today, the state is producing about 950,000 barrels daily, and should be capable of doing so for several years to come (Fig. 3). This volume appears to be ample to take care of all demands over such a period, barring another national emergency. The present production level is the highest in the history of the state, despite the practical elimination of Elk Hills from the picture—a field that contributed much crude oil during the war. California's possibilities for developing new production are by no means exhausted. New petroliferous areas have been proved recently, the ocean off our shores is being surveyed by seismographic methods in a search for hidden geological structures, and some of our fields have secondary recovery possibilities.

Vast supplies of crude oil and natural gas, both proved and yet to be proved, are lying under the surfaces of west Texas and New Mexico, the Rocky Mountains, and Alberta, Canada, all of which are economically contiguous to our Western states. As a matter of fact, a recently completed pipe line from Rangely Field in Colorado to a new refinery in Salt Lake City will supply petroleum products to certain areas previously supplied from California. The fields of Venezuela, and even those of the Middle East, form a further backlog of protection; for example, much Arabian crude oil has already been shipped into the United States and Canada.

The present extensive requirements of the Pacific Coast economy have caused our natural gas supplies, obtained from both oil and dry gas fields, to become insufficient to meet all demands. This condition, however, is being corrected through the construction of pipe lines from Texas and New Mexico to California, and probably from Alberta to the Pacific Northwest. These supplemental supplies of natural gas, which will attain large volume by 1950–51 if announced plans are carried out, will curb demand for liquid fuels, particularly heating and heavy fuel oils, and thereby further reduce the future burden upon California's oil fields.

The growth in population and the general economy of the West have been based upon a solid foundation of rich natural resources and other favorable factors. Further growth may be expected, and petroleum will continue to play its vital role in the destiny of the region.

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RECENT PROGRESS IN TROPICAL MEDICINE

THE Fourth International Congress of Tropical Medicine and Malaria, held in Washington, D. C., in May 1948, was unique among medical meetings in the degree of democracy and fellowship encountered. A large number of the delegates (who came from all parts of the world) were research men who had spent years in the tropics, often in isolated areas, undergoing hardships that would cause the professional explorer to beat a hasty retreat. We have met these men along the steaming Amazon, in the rarefied atmosphere of the Altiplano, and in the isolation of the African veld. They are often difficult to tempt into giving accounts of their personal experiences, but when differences of opinion arose there was no hesitancy in expressing a healthy and well-substantiated conviction on both sides.

For example, the perennial question as to whether syphilis and yaws are manifestations of the same etiology arose following the report of Dr. Leon-Blanco. Men of many nations were drawn into the debate that followed, and the picture is now further confused by the disease pinta being included. Those taking part in the discussion whose homes are in the tropics and who, therefore, have had years of intimate knowledge of these diseases in all their varying forms are of the opinion that we are dealing with separate entities. Dr. A. Carrion, of Puerto Rico, perhaps adequately summed up the matter when he stated:

> It is difficult for me to believe that anyone who has had extensive experience with these conditions can retain any doubt but that they are different. I agree with Dr. Turner that it is helpful to consider these as separate entities. Only when we can culture the organisms on artificial media shall we be able to answer the question of whether these are different species or one species with three varieties.

Dr. Robert E. Johnson and Dr. W. S. S. Landell reported on the adaptability of individuals to tropical climates. They pointed out that in order to avoid mental and physical deterioration during periods of excessive heat, it is necessary to sleep through the cool of the morning. This necessitates late retiring and equally late rising, which should be of interest to all of us in this country, where certain groups have forced Daylight Saving Time upon the working public.

Malaria. Malaria is the perpetual target of greatest interest in tropical medicine and has been the subject of more careful study and investigation than perhaps any other disease known to man. In spite of the vast amount of accumulated knowledge, however, it continues to head the list of causes of sickness and death throughout the world. The economic loss due to malaria is incalculable, and vast areas of fertile land remain idle because of it.

Malaria is a controllable disease, yet it is seldom controlled. The annual death rate in some sections exceeds two hundred per one hundred thousand population per year. Recent work on the Island of Cyprus demonstrates, however, that the disease can really be conquered when there is determination to do so. Here the approach has been to destroy the adult and the larval *Anopheles* mosquitoes by using the newer insecticides and larvicides—particularly DDT. The entire island has been divided into small areas of 3–8 square miles, and in each of these trained men hunt down the mosquitoes in their obscure and often almost inaccessible breeding places. Painstaking diligence has paid dividends. In 1945 in Cyprus, 40 percent of all school children suffered from malaria; in 1948 the rate was only 1.3 percent. The entire campaign is expected to be completed this year at a total cost of little more than a million dollars.

In Holland, too, it has been reported that with the support of the International Health Division of the Rockefeller Foundation malaria control has made great advances. The residual effect of sprayed DDT was found to last for about five months in Holland. Therefore, spraying is done just before the malaria-transmission season begins and is sufficient to control the *Anopheles* mosquitoes for that year. In treated villages and towns, malaria was practically eliminated in 1948.

Cyprus and Holland are only two of many examples in point. In other portions of the world, too, much has been done toward controlling malaria by means of sanitation. Elsewhere, of course, there are still large areas in which proper sanitation is not economically feasible or physically possible. The search must go on for antimalaria drugs with which to treat the inevitable millions of cases. Many cases will continue to occur each year even in the United States.

During World War II, hundreds of potential antimalaria compounds were tested by researchers in the United States, Great Britain, and elsewhere. From the testing there emerged new drugs with undeniably good therapeutic effect. These include such 4-aminoquinoline drugs as chloroquine, such 8-aminoquinoline drugs as pentaquine, and the British-developed benzene-ring compound paludrine.

Chloroquine has in many locations completely supplanted atabrine as the medicine of choice in treating the usual case of malaria. All species of the malaria parasite respond quickly to chloroquine, and only a relatively short course of treatment is necessary. This is in contrast to the six or seven days' treatment required with atabrine. The suppressive dosage of the two drugs is quite different, too; whereas a daily atabrine tablet was found necessary to suppress malaria in malarious areas of World War II, only one dose weekly is required with chloroquine.

Some drugs with a 4-aminoquinoline structure related to chloroquine may prove equal or even superior to it. One of these, called camoquin, has had clinical trials in India, the Philippines, Brazil, and Bolivia and appears to be quite effective in stopping the course of the usual case of malaria with only a single dose of three or four 200-mgm tablets. Further studies on this promising substance are proceeding in these locations and on the Isthmus of Panama, and it is likely that more will be heard about it shortly. Obviously, the availability of a relatively nontoxic drug that is effective as a single dose by mouth will be of untold advantage in the treatment of malaria in rural areas and/or, when necessary, in ambulatory patients. Camoquin is not yet available in the United States.

The Fourth International Congress brought together an enormous accumulation of recently acquired facts on malaria. Colonel H. E. Shortt, of the University of London, School of Hygiene and Tropical Medicine, reported on the obscure development of the malaria parasite during the time which elapses between the bite of the infected mosquito and the entrance of the parasites into the blood. The doctor used the parasite which infects monkeys for his study, and his exhaustive work has cleared up some of the doubtful phases that remain in the life of the plasmodium. He discovered that this intermediate or exoerythrocytic development took place in the liver and he followed this development through the days of its successful evolution.

Hookworm infection: The intestinal roundworms are considerably more of a problem than is generally realized by nonmedical personnel—or even by most physicians. As a matter of fact, hookworm and schistosome infections rank next to malaria as the most dangerous diseases of mankind. (A brief concept of the magnitude of the hookworm problem even in the United States was conveyed in the March 1949 issue of THE SCIENTIFIC MONTHLY.) Laughlin and Spitz, in a recent article in the *Journal of the American Medical Association*, have estimated that one in every fifteen servicemen on Pacific duty returned to the United States with the "Old World Hookworm," *Ancylostoma duodenale*. There is a distinct danger that this hookworm, which is more resistant to treatment than the "New World Hookworm," *Necator americanus*, may become firmly established in this country if cases are not discovered and treated as soon as possible.

Dr. W. O. Cruz has ably defended the conception that hookworm anemia is a deficiency disease and can be combated effectively by correcting nutritional deficiencies.

Generally, there is some reluctance on the part of physicians and laboratory personnel to thoroughly examine the stool specimen under the microscope for worm eggs. As Laughlin and Spitz state, "The extensive contact with malodorous excrement is usually enough to stifle the enthusiasm of the most ardent diagnostician." These investigators have described a method of stool examination which concentrates the worm eggs in a stool specimen by making use of certain proportions of saline, aerosol, ether, and xylene. It seems a rather better method than other concentration methods which use brine, zinc sulphate, etc. With an ordinary fecal smear, at least 1,200 hookworm eggs per gram of feces are required before detection is regularly possible, but with the "AEX technique" very light infections may be picked up.

It is fascinating to note that, in the midst of warnings and admonitions about the seriousness of hookworm disease and the necessity of early diagnosis and treatment before onset of significant anemia, a therapeutic use for the disease should be found. But such is actually alleged to be the case. A report appearing in the *Indian Medical Gazette* describes the use of artificial infections with *Ancylostoma duodenale* for the treatment of the disease polycythemia, an abnormal increase in the number of red blood cells in the human blood. The Indian physicians use 300–600 hookworm filariform larvae and apply them to the skin of the patient under wet blotting paper so that they will penetrate the skin. In due time, the hookworms reach the intestine where, as mature worms, they each account for almost a cubic centimeter of blood loss daily, which offsets the patient's polycythemia. Instead of causing anemia, as it otherwise would in a normal individual, in polycythemia patients the hookworm infection produces a rather normal hemoglobin value and red blood cell count. This type of treatment has been called "ancylostoma-

therapy," and has been used in about twenty-five patients with good results. It is a simple, long-lasting type of treatment because the duration of the untreated hookworm infection in the human body is five to six years.

Filariasis: Filariasis is the term applied to the infections in humans and animals caused by various small roundworms. Most generally, however, it is applied more specifically to the particular disease of humans caused by the worm *Wuchereria bancrofti.* Filariasis, when referring to the illness produced by *W. bancrofti,* is mosquito-transmitted. Ordinarily, it is a self-limiting febrile illness, and it is only when infection occurs repeatedly that serious symptoms occur. The mature larvae and the adult worm, passing through tissues, especially through lymphatic vessels, cause blockage of the lymph vessels and/or local reactions. Enlarged glands in the inguinal or groin region may occur, or there may be swelling of the testicles. Very severe cases in natives occasionally give rise to such a marked lymphostasis that a whole leg may become markedly enlarged and disfigured, a condition popularly known as "elephantiasis." During World War II, filariasis became a serious problem among servicemen in certain areas, mainly because widespread rumors arose that men so afflicted would become sexually sterile. Such false rumors caused considerable unnecessary consternation among the troops.

In spite of the fact that only repeated infection with filariasis will produce permanent swellings in the groin, scrotum, or leg, it seemed desirable to treat the cases that occurred among servicemen, although no satisfactory drug was known. The pentavalent antimony compound, neostibosan, and the trivalent antimony compound, fouadin, were used, but the results were not remarkable. Very recently, however, there has come to knowledge a substance called hetrazan (1-diethylcarbamyl-4-methylpiperazine hydrochloride) which appears to be quite promising. Treatment of patients in Puerto Rico resulted in either complete disappearance of all the worms or their reduction to very small numbers. In general, the worms completely disappeared from those patients who had received higher dosages. In none of the treated individuals was there any evidence of the disease in the lymphatic vessels, nor any other symptoms suggesting clinical filariasis. Hetrazan, therefore, seems about to take a conspicuous place in the treatment of one more of the previously well-nigh untreatable tropical maladies.

Typhoid fever, although not strictly a tropical disease, is of primary importance in the tropics. Early experimental work, proving that chloromycetin is a highly effective remedy against this disease, has been amply substantiated. This adds another disease to the credit of this new antibiotic, in addition to typhus and scrub typhus, for which it is so highly specific.

The steady advance in the therapy of Hansen's disease continues as new drugs prove their effectiveness. Before the end of the present year we shall hear of new compounds which produce results in a shorter time and are free from unwanted side reactions. Everyone engaged in work with Hansen's disease is grateful for the enlightenment of the public toward this malady. The superstitions and unfounded fears that have been associated with it are gradually disappearing, and the public is beginning to understand that Hansen's disease is nothing mysterious, but a chronic disease difficult to contract and of less danger to the public at large than the common cold.

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BOOK REVIEWS

SINOLOGY

History of Chinese Society: Liao (907–1125). Karl A. Wittfogel and Feng Chia-sheng. xv + 752 pp. Illus. $12.50. Macmillan. New York.

THE appearance of this huge book by Wittfogel and Feng ushers in a new era of research in Chinese history. Although the present work covers a very short period of Chinese history (a little over two hundred years) and though, even in this period, it covers only the recorded events of the alien dynasty which ruled a portion of China, the contributions of this volume, first of several to come, are extremely significant.

For the first time, the authors have made available to Western scholars who cannot read Chinese, and to Chinese scholars who do not have the necessary background or time, a vast body of material, classified, annotated, and interpreted in accordance with a particular theoretical framework arising from Dr. Wittfogel's many years of thought and study. The most important theoretical conclusion of the present volume has been explained very clearly by Wittfogel in his General Introduction. For many decades it has been commonly held by Sinologists, including even Professor Pelliot of France, that conquerors of China in historical times have always been completely absorbed by the Chinese. The materials presented in the present volume show how ill-founded such a view is. The authors have shown that rulers among the Chitans, founders of the Liao dynasty, not only resisted Sinicization, but they also attempted extension and expansion of their own cultural norms and ideals. In the end, when they no longer could reign in China, they preferred to return to tribal life in order to maintain their identity, instead of taking on a mode of life characteristic of the Chinese.

Some readers may raise two possible lines of objection. The first is an old one, and has been current among a number of Chinese scholars since Dr. Wittfogel visited China in the early thirties. These scholars maintained that the Chinese classics are too complex for any schematic treatment and that Dr. Wittfogel has merely been forcing the data into preconceived categories. This view is untenable for two reasons. First, those who have criticized Wittfogel in this vein have nothing better to offer. Second, science progresses by a race between data and theories, with the one sometimes ahead of the other. All scientific research involves some categories or hypotheses which must be tested by the data subsequently collected. To collect data one must have some theories; but to elaborate and maintain the theories one must have adequate data.

The second line of objection can be more vital. Some readers will think that Wittfogel and Feng have greatly exaggerated their case. These scholars may observe, for example, that most of the activities of the Chitans recorded were those of the ruling group, that many of the facts have merely been assumed from legislation or imperial orders, that no two peoples in contact with each other as were the Chitans and the Chinese have been known to retain completely their original identity, and that, therefore, although the "complete Sinicization" theories are questionable, the "complete resistance" theory is equally unscientific.

There is probably no effective defense against this criticism. What we can say, perhaps, is that Wittfogel and Feng's work represents a reaction against the theories so long current in Sinological thought and that in time a better-balanced theory will emerge, with the two extremes serving as checks on each other.

The present volume will be welcomed by all students who are connected with work on China in general and that on Chinese history in particular. The work will stand for many years to come, not only as an important source of inspiration but also as a standard against which future works will be measured. The reviewer, among others, will be anxiously waiting for the appearance of other volumes of the Chinese History Project, under the directorship of Wittfogel and with the able collaboration of such scholars as Messrs. Chu Tung-tsu and Wang Yu-chuan, and Mr. and Mrs. Fang Chao-ying.

FRANCIS L. K. HSU

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THE EVOLUTION OF PHYSICS

From Euclid to Eddington. Sir Edmund Whittaker. ix + 212 pp. $4.00. Cambridge University Press. New York.

THIS book, as the author says in his preface, is not intended to be a summary of present-day knowledge in physics, but rather a history of the evolution of concepts and principles, especially such as have provoked long controversies, in some cases still unsettled.

In the course of this evolution there have been several developments of the first magnitude in importance, such as non-Euclidean geometry, relativity, and quantum mechanics. The author describes and discusses these developments in considerable detail, with some interesting comments on certain points. For instance, in discussing determinism, which asserts that all events in a physical system take place *causally*, he says that whether this assertion be true or false is

a question which can be decided only by a theory based upon, and tested by, observation. He goes on to say: "We may ignore all the metaphysical nonsense that has accumulated in this connexion." As an example of such nonsense he cites in another place a statement by Whitehead that from the philosophical point of view a predicate need not have a subject; in other words, that motion may take place without anything being moved; and that therefore there is no necessity for inventing an ether to transmit light waves through a vacuum.

The concepts of quantum mechanics are discussed by the author in considerable detail; and in this part of the book he finds it impossible to get along without mathematical equations, which he has rather successfully avoided in earlier sections.

In the concluding part of the book the author discusses Eddington's account of the structure of the universe, and closes by saying that the work of unification effected by Eddington may be compared with the great reform achieved by Maxwell seventy years earlier when he correlated electrical and optical phenomena on the basis of a single ether. The author states that Eddington's work has brought into relation the domains of general relativity and quantum mechanics, and has thus laid the foundations of a unified doctrine of nature.

PAUL R. HEYL

Washington, D. C.

SCIENCE AND MOTION PICTURES

Painting with Light. John Alton. 191 pp. Illus. $6.00. Macmillan. New York.

LIKE most things out of Hollywood, John Alton's *Painting with Light* is a slick job of telling how he and his contemporaries achieve their triumphs.

This 191-page book is crammed with nearly 300 illustrations. The author is a member of the charmed group of the American Society of Cinematographers. Mr. Alton covers every angle and problem with which a camera man or lighting expert would be confronted during the course of filming a picture. Every addict of 35-mm, 16-mm, or 8-mm cameras would do well to have this book in his library.

Actually, Mr. Alton not only presents problems but solutions to them, as in the chapter on Mystery Lighting. This portion should attract the attention of still-camera men, illustrators, and pictorialists. It is broken down into such subjects as shipwrecks, flashes of guns in absolute darkness, street lights, fireplace scenes, foggy nights, and the all-important low key shot of doubles, where the Actor's Guild requires the employment of acrobatic doubles.

The book continues with an endless number of short, concise hints, either to do or not to do, always tempered with moderation. I quote, "Snow scenes can be beautiful but for good results shoot in the early morning, even before sunrise or late in the evening. In the time between the light is flat." He goes on to say, "Nobody in his right mind would think of taking a picture in similar light inside, so why make the mistake outside?" He adds, "The wind, haze, mist, fog and storm are all elements that make snow scenes beautiful. Those are your colors. The sun is your brush, so go ahead and paint."

Tribute is paid to the late, great Alfred Stieglitz for his outstanding still portraits, noting that it took the film industry a long time to invent the motion-picture close-up.

The Hollywood Close-Up chapter is most thorough. It deals with camera angles, illumination, composition of subject and background, choice of lenses, make-up, and important tips on how to feature or suppress good or bad features of a subject.

This reviewer hopes that, someday, someone will turn out an equally fine book concerning the problems of the still photographer.

A. AUBREY BODINE

Camera Magazine
Baltimore, Maryland

SCIENCE AND ART

Art as the Evolution of Visual Knowledge. Charles Biederman. xi + 696 pp. Illus. $15.00. Charles Biederman. Red Wing, Minn.

MR. BIEDERMAN is a creating artist. Both his interest in science and his attitude as a writer appear to be much influenced by his efforts and enthusiasms as inventor and advocate of a new plastic style. He "rejects" both the academic and modern viewpoints as evasive of present responsibilities and finds his particular type of "nonobjective" art the only legitimate direction for the "non-camera" artist today.

The format of the book is impressive. Quarto, with a light-blue cloth cover and stamped white title, it is heavy and a trifle awkward. It is profusely illustrated with excellent half-tone reproductions of subjects from leading museums or printed by special permission of publishers of authoritative books on art, history, and science. These illustrations include human creations from Mousterian cupstones to photographs of X-ray diffraction patterns of urea oxalate. Of many works of individual artists shown, two are by Michelangelo, four by Leonardo, six by Monet, sixteen by Picasso, and sixteen by Biederman.

The artist-author's inventions in painted wood, metal, and plastic, when pictured beside X-ray diffraction patterns, crystal and molecular models, and mathematical string models suggest his fascination with science and his inspiration from certain of its visual aspects. One composition, called "White Construction with Colored Lights" ($54\frac{1}{2}'' \times 52'' \times 12\frac{1}{2}''$), made from painted glass and wood with hidden fluorescent tubes, is installed in the Interstate Clinic, Red Wing, Minnesota. The visual pleasures afforded by the fine reproductions of Mr. Biederman's "constructions" are apt to appeal to many readers as

more convincing than any satisfaction of be derived from his justification of "Constructionism." His book shares the compositional ingenuity of his visual arrangements, but his words often lack their precision and clarity. He constructs his novel reorganization of art history in two very unequal time divisions, to which about equal amounts of space are devoted. Art before 1840 is separated from that of the present by a smaller third division of eighty pages on the fifty-year period between the invention of the camera and the innovation of cubism. In the author's opinion, this crucial half century effected the transition from the age-long struggle of the artist to record the appearance of nature literally (with innumerable regressions such as the period of Christian art) to the present in which the artist strives to create art, not "imitate" the art of nature.

A bibliography of 374 titles, many of distinction, is listed, from which quotations are liberally appropriated and freely applied. Sometimes these are interspersed between sections, sometimes included in the text. The former, coming in short series, produces a montage of impressions of the meaning to follow. The latter is often a clarification of it, as in the following example:

> Man must conform to the structure and material function of the organism and environment, the factors which constitute "reality," if he is to be able to manipulate and exploit that reality for his own purposes; in fact, the extent of his ability to do so depends precisely upon his ability to conform to the structural function of the world process. This Francis Bacon recognized in the 16th century when he wrote: "In order to master Nature we must first obey her."

Had the ardor of the artist been tempered with scholarly frugality, a much-compressed work might have said more. Mr. Biederman is his own publisher.

THOMAS M. BEGGS
National Collection of Fine Arts
Smithsonian Institution

INTRODUCTION TO ANTHROPOLOGY

Man in the Primitive World. E. Adamson Hoebel. xii + 543 pp. Illus. $5.00. McGraw-Hill. New York.

ONE index of the rapid growth anthropology has undergone in recent years is the number of general textbooks published. Four appeared in 1948, and two during the current year, a total far greater than in any previous comparable period. Of these, Hoebel's introductory text is among the very best.

In the author's own prefatory remarks,

> Anthropology must bring its conclusions to bear upon the problems of modern society; it must place its methods at the disposal of all the other sciences. Nevertheless, its great contribution to knowledge has been derived from its special quality as a comparative science. . . . It is still best to keep anthropology firmly rooted in the data of primitive society. . . . In keeping with this conviction, this book is primarily a study of man in the primitive world.

Accordingly, Hoebel has provided the student with a balanced series of chapters treating of human paleontology and prehistoric archeology, physical anthropology, the various aspects of primitive (*i.e.*, nonliterate) society, and the relationships of society and culture as expressed in culture areas, personality, invention, diffusion, and cultural evolution. Pertinent examples are given throughout, mainly from North American Indian tribes. On questions of theory and interpretation the author has maintained a strong measure of eclecticism. Understandably, no one person can be a master of all the many branches of so vast a discipline as anthropology; Hoebel's particular forte has long been primitive property, law, and government, and the chapters devoted to these topics are a real contribution to other professionals as well as to the student reader.

One omission in this text, of consequence particularly if it were to be used by underclassmen, is that Hoebel nowhere provides an extended discussion of the learning processes, as formulated in psychology, and on which rests the acquisition of cultural behavior among humans. The professional readers can discern, from numerous passages, that here, too, the author is eclectic. But it is one of the problems of teaching introductory anthropology to provide the student with a coherent psychological framework in which he can view behavior, and its development in individuals, in societies whose cultural patterns are often radically different from his own.

A glossary of technical terms, names, and concepts forms a valuable adjunct to this work; two things lacking that would have further enchanced the book's usefulness to students are a table of figures and the inclusion of the glossary items in the general index, for, although the various terms all appear in the body of the text, the glossary does not give page references. Several unfortunate typographical errors somehow crept in: it is *Bogaras,* not "Bogarus;" *Herskovits,* not "Herskowitz;" *Montagu,* not "Montague;" *R.M.,* not "R. I.," McIver; *S.D.,* not "S.O.," Porteus; and in some mysterious fashion Ralph Linton has acquired the middle initial M. which appears in none of his writings, Library of Congress cards, or biographies.

Style of binding, format, and slick paper combine to give the whole book the not unpleasant air of "solidness" characteristic of McGraw-Hill texts.

D. B. STOUT
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THE TIMES WE LIVE IN

Reflections on Our Age. 347 pp. $4.50. Columbia University Press. New York.

THIS volume consists of twenty-two lectures delivered at the opening session of UNESCO in Paris in the latter part of 1946. It was hoped that some of the speakers would develop ideas that would guide the conference in its cultural activities, and that others, because of their eminence, would stimulate

public interest in the work of the meeting. As is customary in such enterprises, there is a considerable variation in the quality of the papers.

There appears to be such a strong determination on the part of UNESCO to avoid the evils of this world that some of its aims may occupy its energies for many years. Because of exchange controls and other impediments, the scholars of many countries find it impossible to acquire the books and journals they need; there are embargoes on the acquisition of works of art; scholars, particularly archeologists and anthropologists, find that they cannot visit parts of the world that they must see in order to carry on their work. One of the purposes of UNESCO, and a seemingly practicable one, is to moderate this heritage of World War II. It has set itself the further aim, however, once the scholars of the world are set free from the burdens that now oppress them, of assisting in the production of studies that will conform to ideals of accuracy and truth. To assist in carrying out this purpose UNESCO proposes to establish a World Library Center in Paris, the facilities of which will be available to scholars everywhere.

Thus, as Mr. David Hardman points out in his introduction to the present volume, we may look for an improvement in textbooks in history, geography, and civics. Instead of six versions of a military campaign we will have "one authentic, documented account." This idea was further developed at the Mexico City meeting of UNESCO in 1947, where it was pointed out that many of the troubles of the world were due to the promulgation of bad philosophy. It was therefore proposed by an American delegate that the philosophers should resolve that henceforth only true philosophy should be written. This idea has a respectable ancestry, beginning with Plato's observations on the dangers inherent in permitting poets to propagate myths, and including the progressive Wisconsin statute passed in 1923 which forbade the use in that state's schools of any textbook which falsified the facts with respect to the War of Independence or the War of 1812, or defamed the nation's founders, or misrepresented the ideals and causes for which they struggled, or which contained propaganda favorable to any foreign government.

Many of the speakers represented in the present volume were not members of UNESCO, and were thus free to expound their ideas without reference to UNESCO's program. M. Malraux, a de Gaullist, put forward a theory of art which aroused the utmost scorn of M. Aragon, who has identified himself with the Marxist point of view. Mr. Ayer, a leading nominalist, explained why all past and present realist and idealist philosophy is in error, and Mr. Herbert Read, also a nominalist, showed why all past aesthetic interpretation must be discarded. For the most part the scientists contented themselves with brief, excellent accounts of progress in our knowledge of such fields as the submarine underworld, the physiology of the nervous system, cave paintings, and genetics. The volume also contains a moving plea by the Greek representative on behalf of the claims of ancient Greek rationalism to a place in the deliberations of UNESCO.

HUNTINGTON CAIRNS

National Gallery of Art
Washington, D. C.

BOTANICAL EXPLORATIONS IN THE FIJIS

Naturalist's South Pacific Expedition: Fiji. Otto Degener. [8] + 303 pp. Illus. $5.00. Otto Degener, Waialua, Oahu, Hawaii.

OTTO DEGENER presents a curious mélange of his experiences in collecting plants in the Fiji Islands in 1940–41, with accounts of the past and present customs of the natives of these islands and their present condition under colonial British rule. His botanical collecting in the Fijis was under the auspices of the New York Botanical Garden and of the Arnold Arboretum and appears to have been eminently successful. (One is intrigued by the somewhat obscure arrangements whereby it seems that he is to inherit the junk-yacht *Cheng Ho* from its former owner, Mrs. Anne Archbold, the sponsor of the Cheng Ho Expeditions.)

The account of botanical collecting gives a thoroughly interesting picture of the work of an exploring and collecting botanist in the tropics, which is sometimes a little overtechnical or lacking in explanations for the nonbotanical reader. There are glimpses of shell collecting and of other zoological interests, but these are unfortunately brief. There were two collecting stations on Vanua Levu, and several on Viti Levu, including the slopes of Mount Evans. The results included the discovery of a remarkable new family of plants, the Degeneraceae, about which Mr. Degener is, perhaps pardonably, somewhat naïvely vain.

The anthropological information about the Fijians of the last century is a well-written review of the source material. Various accounts of present-day Fijian customs, like the drinking of *yangona*, the women's "sitting dance," and the methods of house construction are at firsthand. The information about medicinal and food plants is valuable.

The accounts of race prejudice, "white supremacy," and other failures of the British colonial rule seem to be painfully firsthand. Nevertheless, the status of the Fijians in the all-Fijian communities seems to be not without dignity and independence, and the amount of racial intermixture remarkably small. One may wonder what may be the impact of the Fijians' distinguished war service on the three-way race problem presented in the relations of whites, Hindus, and native Fijians in their island isolation.

KARL P. SCHMIDT

Chicago Natural History Museum

ATOMIC ENERGY IN PEACETIME

Radioactive Tracer Techniques. Geo. K. Schweitzer and Ira B. Whitney. vi + 241 pp. Illus. \$3.25. Van Nostrand. New York.

THIS is the first laboratory manual on radioactive tracer methods the reviewer has had an opportunity to examine. In general, it seems to be a satisfactory text, but it does leave a great deal of material to be derived from the lecture course. The first four chapters are devoted to general consideration of health hazards, laboratory construction and conduct, shielding, and such matters, which are of considerable importance in orienting a beginner, but the authors have failed to include quantitative data which would add much to its value. For example, the magnitude of natural radiation exposure and data on stopping power of various shielding materials would be of interest. The novice would be helped by the inclusion of a few tables of pertinent data.

The hazard of experiment contamination merits more attention than is given in this text—the exposition of the concept of contamination potential is left to the instructor. A problem or two of this nature would increase the worker's respect for rules and regulations.

The discussion of the experiments is clear, although perhaps others might have been selected. Putting a beginner to work measuring zinc, for example, is giving him a tough assignment.

Surgical gloves and their proper use are of manifest importance, but other gloves, heavier ones in particular, have their place and are too generally overlooked.

Constructive Uses of Atomic Energy. S. C. Rothman, Ed., ix + 258 pp. Illus. \$3.00. Harper. New York.

THIS volume is intended, say the publishers and editor, for the intelligent layman; its purpose is to assure him that really constructive uses of atomic energy are being actively developed. The papers chosen range from aircraft engines and atomic power for industry through the fields of biology, ceramics, and medicine to soil research, with an impressive array of authors.

The first chapter is based on Dr. A. H. Compton's Franklin Medal Lecture, which merits a greater audience than is reached by the *Proceedings* of the American Philosophical Society. Dr. Compton discusses the problem of establishing international order and, in general terms, the developments to be expected and problems to be met in the peaceful utilization of this new source of power. Although Dr. Compton might be less optimistic today (the lecture was given in November 1945) of the effectiveness of the release of atomic energy in "compelling man to become more human," his presentation of the case deserves thoughtful reading.

Dr. L. W. Chubb, in the second chapter, gives an excellent account of the development of concepts of atomic and nuclear structure from Democritus, 400 B. C., to the present.

A brief chapter by S. K. Allison serves to introduce the eleven chapters on specific peacetime applications of nuclear energy and radioactive by-products.

Chapters IV and VI, dealing with the industrial applications of, and chemical process control with, radioactive materials, will interest many readers familiar with instrumentation and control problems of industry. The fifth chapter, entitled Atomic Power for Industry, which gives a very readable account of the present status of power reactor development, as of July 1948, and Chapter VIII, on Atomic Engines for Aircraft, will clarify many a layman's idea of just what atomic power means in practice.

Most readers will probably find the papers on Radioactive Materials in Soil-Fertilizer Research, The Use of Tracers in Biology, The Medical Uses of Atomic Energy, and Radioactive Tracer Techniques in Pharmaceutical Research the most interesting and understandable, these chapters being written, for the most part, with considerable skill in nontechnical definition of the problem and the mode of attack.

The chapters on Radioactive Tracers in Metallurgical Research and Ceramics and Nucleonics, although interesting, are rather heavy going for the uninitiated.

The text closes with a brief (4-page) chapter on Nuclear Physics and Medical Research.

The book has two appendices that should be of particular interest to the nonscientific reader. The first is Chronological List of Highlights, the second A Glossary of Terms Used in Atomic Energy.

The printing and format are good, and illustrations, photographs, and sketches interesting and well captioned.

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CANADIAN SCIENCE

A History of Chemistry in Canada. Compiled by C. J. S. Warrington and R. V. V. Nichols. x + 502 pp. Illus. \$4.50. Pitman. Toronto.

IN A little less than half a century, Canada has changed over from a predominantly agricultural country to one of the major industrial countries of the world. With the wealth of her natural resources, the impact of chemistry on the growth of Canadian industry has been outstanding, and the book under review is a faithful record of what has taken place during and before this period. Compiled from the data supplied by specialists in industry, the universities, and the public service, the text is more than a rearrangement of the material submitted by various sources. The sequence of the chapters is such that there is a remarkable continuity, leading from the chemistry of metals, mineral products, coal, and petro-

leum to that of the organic substances derived from wood, agriculture, and fisheries, and, finally, the synthetic products derived from coal, air, and water. The last chapters are devoted to the public services, research institutions, chemical organizations, journals, and the teaching of chemistry. This arrangement provides an entertaining story of the development of chemistry in Canada that can be read by the layman as well as by the chemist. Descriptions of processes are particularly well written, and due credit is given everywhere to the individuals, teams, and companies who devised such processes. The book is well illustrated with half tones, line drawings, and maps.

Chemical research, mostly applied, is noted throughout the book, but there is no special chapter devoted solely to research in pure science. Only brief mention is made of the main lines of research in which notable advances have been made. This would lead one to believe that, had the authors tried to describe such work, the size of the book would have been considerably greater. Let us hope that, someday, a comprehensive story of this work will be written with the same objectivity and the same degree of accuracy found in this fine contribution to the history of chemistry in North America. The authors, the Chemical Institute of Canada, and the donor company (Canadian Industries Limited) should be congratulated on this successful effort.

LÉON LORTIE

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ENTOMOLOGY

Traité de Zoologie. Anatomie, Systématique, Biologie. Tome IX: *Insectes. Paléontologie, Géonémie, Insectes Inférieurs, Coléoptères.* Pierre-P. Grassé, Ed. 1,117 pp. Illus. 4,500 fr. Libraires de L'Académie de Médecine. Paris.

THIS is the first to appear, the second in contemplated sequence, of three volumes on insects of the *Traité de Zoologie*, which will eventually comprise seventeen volumes and cover the entire animal kingdom. The work calls to mind the ambitious efforts of Cuvier and Buffon.

The present volume discusses the geographical distribution of insects, the classification of fossil insects, the minor orders of insects, and one major order, the Coleoptera. The authors of this part, Chopard, Denis, Despax, Jeannel, Paulian, and Grassé, (the editor for the entire section on entomology), are all names well known in entomological literature.

Jeannel's opening section on the classification and phylogeny of fossil insects is a concise summary of the present knowledge on the subject. It is well documented and copiously illustrated. This is followed by the same author's discussion of the geographical distribution and evolution of insects, which is a condensation of the views set forth in his separate work, *La Genèse des Faunes Terrestre,* and is in turn based upon the theories of Wegener.

The treatment of each order or group of related orders is by a French specialist in the group. The plan followed under each consists of a general diagnosis of the group, a comprehensive description of the forms included, an extensive section on the morphology, and a section on the systematics, which covers all the families of the order, as recognized by the worker writing the part. Each part is illustrated with an abundance of figures of the particular structures and of the gross habitus of numerous species of insects. The excellent illustrations, many published for the first time, are one of the really outstanding features of the work.

Some of the associations and divisions, as well as the names used, will not be familiar to all workers, but the essence of the system used may be found by reference to the *Table des Matieres* on pages 1110–17. The reasons for the arrangement and use of names will generally be found in the discussions and by checking the papers cited in the bibliographies.

Each section is followed by a rather extended bibliography, which contains many of the titles of the most recent papers that have appeared. These bibliographies will prove as useful as the many illustrations.

Grassé's discussion of the termites is quite extended and illuminating, covering approximately 140 pages. When one stops to consider the importance of the group from an economic standpoint, as well as from the standpoint of their interesting social habits, this is not an overemphasis.

The treatment of the Coleoptera by Jeannel and Paulian occupies about one third of the volume. The number of recognized families is greater than in any American work, but higher categories in taxonomy are a matter of personal preference. Their division of the order into suborders and superfamilies invites consideration by American workers.

The other large complex treated in this work is the Orthopteroids. The inclusion of the Plecoptera and Embidina in this superorder seems strange in view of the rather evident relationships both have with the Blattaria.

The book will have definite value as a reference work for the teaching entomologist, and it will be highly useful to the general systematic worker as a resumé of the current ideas of the French specialists involved in the undertaking.

JOHN G. FRANCLEMONT

Arlington, Virginia

BRIEFLY REVIEWED

Our Sun. Donald H. Menzel. vii +326 pp. Illus. $4.50. Blakiston. Philadelphia.

THIS is the eighth and latest in the series called "The Harvard Books on Astronomy," written for the amateur astronomer and others interested in the

latest theories of astronomy. Moving pictures of the sun taken through special filters have been studied by Dr. Menzel and his associates, resulting in revised solar theories through an increasing knowledge of the sun. The book makes use of a new term: "spectroheliokinematography." Not only is the word new since 1938, but the use of the instrument, which is a moving-picture technique applied to the older spectroheliograph, illustrates the difference between methods used for a study of the sun's disk only eleven years ago and at the present time.

The book will be more interesting if the reader has seen the 16-mm film *Explosions on the Sun*, made at the Climax, Colorado, station of the Harvard Observatory, or similar films made at the University of Michigan. He will then understand much better the discussion of solar prominences in Chapter 8. The reader will also be intrigued by a review of the latest theories of sunspots as given in Chapter 6. In fact, he may disagree so completely that he may want to formulate his own theory.

The book is fascinating reading, with excellent illustrations and rather thorough descriptions of solar phenomena, but it is not always too easy to understand. No attempt is made to discuss completely the mathematical and physical theories that the expert will demand.

The book is a *must* for all amateur astronomers. It is to be hoped that it will inspire the amateur with a telescope to make systematic sunspot observations and to try to correlate his observations with observations of aurora displays and radio fadeouts.

C. M. Huffer

Washburn Observatory
Madison, Wisconsin

The Ways of a Mud Dauber. George D. Shafer. xii + 78 pp. Illus. $2.50. Stanford University Press.

THE author, a retired professor of Stanford University's Department of Physiology, became intrigued with some white pellets that were visible through the body wall of the larvae of a wasp. He wanted to know about these and set about with scientific acumen to find out what they were and why. They were really uric acid crystals, as he learned in a short time, but he became so engrossed with wasps that he devoted five years to an intensive study of their physiology, their metamorphoses, and their habits.

The result is an exhaustive, but not exhausting, life history of the same mud daubers. Combined with a flair for ascertaining facts, Professor Shafer developed a sympthetic and affectionate regard for some of the objects of his studies. One of them, Crumple-Wing, "a pot marred in the making," had a useless wing. The professor actually tried to replace this by cementing the wing of another wasp in place of the abortive one. This did not work, but Crumple-Wing continued her life as normally as she could with this handicap, and when she finally died the professor dedicated his book to her.

It is interesting reading, both for the layman and for the entomologist. Perhaps some other students will find out more about the life of these insects than Shafer did, but it will take them a long time and a great deal of careful observation. I enjoyed the book.

W. M. Mann

National Zoological Park
Washington, D. C.

The Epitome of Andreas Vesalius. L. R. Lind, Translator. xiv + 133. Illus. $7.50. Macmillan. New York.

FOUR centuries ago, Andreas Vesalius of Brussels fathered the science of modern human anatomy. From the work of this great pioneer observer stems much of modern objective biological science. Vesalius' major work, *De Humani Corporis Fabrica* (1543), is immense; his *Epitome* is a masterly condensation of a vast amount of material. Dr. Lind's translation is faithful to the original Latin, yet uses modern technical phraseology. In addition to the complete translation, beautiful plates give us the complete Latin text, and all the meticulous woodcuts of the original.

The volume is beautifully printed, the illustrations are excellent, and the clear, precise translation is a pleasure to read. For all those interested in the history of the development of science, medicine, and, particularly, human anatomy, this first complete translation of the work of the famous master is a true treasure.

Edward J. Stieglitz

Washington, D. C.

CORRESPONDENCE

SENSE AND NONSENSE

That the man who wrote with such charm and good humor on *The Lungfish, the Dodo, and the Unicorn* failed to respond to the charm and good humor of Matt Kahn's drawings in your May issue is in itself arresting. Beyond that, however, we are reminded of the continuing need for a zoology and biology of the scientific temperament.

Perhaps when the breeding, feeding, and organizational habits of scientists have become "textbook stuff" we will have a differential physiology for those who think *The Conquest of Space* nonsense and those who think "glamour girls in four colors" nonsense. Only then would it be possible for Willy Ley to write, as only he could write, a study of mythical, rare, and extinct types of scientists.

With apologies to him a sample item might read . . .

> To this day there are occasional reports scattered over America of a rare type of scientist who takes an interest in art. Typical is the example cited by an observer in Manhattan:
>
> "Shortly after my publication of *The Application of Boolian Functions to the Sex Life of Economists* (Brown and Bigelow, $4.00), I chanced to be in an art museum in upper Manhattan. I encountered one of the strangest things I have seen. A man and woman entered the room where I sat ruminating. I recognized the male at once as a Ph.D. in organic chemistry I had often seen on the campus of a nearby university. As near as I could judge, this specimen was of the somatotype 1–3–6, quite tall and aggressive in stance.
>
> "The two stopped before a Picasso oil—'The cube of a nude,' to be exact. I distinctly heard him say, with the bleating tones these specimens often use in the presence of the female, 'I have always loved this painting.'
>
> "My astonishment was so great that I had no thought of capturing the specimen. Later when I sought him out on the university campus I found that he had migrated elsewhere.
>
> "This incident left no doubt in my mind that there exists a very rare type of organic chemist with the somatotype 1–3–6 who likes Picasso."

As a subscriber I enjoyed Matt Kahn's drawings as much as Willy Ley's book.

WYMAN GUIN

Lakeside Laboratories, Inc.
Milwaukee, Wisconsin

ERNIE AND PHYSICS

Whenever Ernie leans against a wall
He thinks of how he holds it up as much
As it holds him, and, suddenly grown tall,
Confronts the world without a mental crutch.

FRANCIS BARRY

Philadelphia

PRAISE AND BLAME

The September number of THE SCIENTIFIC MONTHLY has so far failed to reach me, although I notice it is out. I would appreciate it if you could trace this copy or mail a duplicate to me. I should hate to miss it, as I find it of extraordinary interest, combining as it does so skillfully, presentation in terms accessible to those outside the special fields covered, of articles reporting the latest scientific developments. I enjoy, too, perhaps for mildly "escapist" reasons, such accounts as "The Hummingbirds' Brook," in the December 1946 issue, and "A Botanist's Dominican Diary" in March and April 1944.

H. P. DUTTON

Evanston, Illinois

"New Permanent Antifreeze" (*Sci. Mon.*, 1949, **69**, (4), iv). I object to tripe like the above in a serious scientific publication—unworthy of a Hearst newspaper. And it is not the first time this column has made poor selections.

ARTHUR BARRY

Greenville, Delaware

BACK NUMBERS

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